THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

An Account of M. Bessel's *Pendulum Experiments.*

The volume of the transactions of the Berlin Academy which is just published contains the very important memoir of M. Bessel on the length of the seconds pendulum at Königsberg.

Having had an opportunity, in the summer of last year, for which I am greatly obliged to Professor Schumacher, of examining M. Bessel's apparatus, set up for that purpose in the observatory at Hamburgh, with the advantage of the presence of the eminent artist, M. Repsold, by whom it was made, and furnished with M. Bessel's memoir then unpublished, I trust that the following early notice of its valuable contents may not prove unacceptable to those who in this country are interested in the same pursuit, and are still awaiting the arrival of their long expected copies. To such it will not supersede the perusal of the work itself, abounding as it does in original and profound discussion, on a subject which has numbered, and still numbers, amongst those who are engaged in it, the most distinguished names of continental science. To more general scientific readers, it will convey some knowledge of an important work, of which probably not more than a few copies will be in circulation in this country, and those in a language (the German) which is not so generally read amongst us as it deserves to be, although becoming more so every day.

M. Bessel's memoir may be divided into two general heads: first, the measurement of the seconds pendulum at Königsberg, by a new method, invented by himself; and, second, dissertations and experiments on points connected with pendulum experiments generally.

The principle of M. Bessel's method of determining the seconds pendulum bears a near resemblance to that of our ingenious countrymen, and early experimentalists in pendulums, Hatton and Whitehurst. It consists in the application of a moveable point of suspension to a pendulum, composed of a brass ball and metallic thread. The brass ball remains always at the same level, and the metallic thread is shorter or longer according as the suspension is brought nearer or removed further from the ball. The effect is thus produced of two pendulums, of which the difference in length is known, being the interval between the points of suspension in the two cases; the interval being made, mechanically, equal to the distance comprised between the extremities of a standard bar. The ratio of their lengths is also known, by observing the number of vibrations performed by each in a given time. Whence the time of vibration due to the length comprised between the points of suspension in the two cases, *i.e.* to the length of the standard bar, is obtained; and the length of the seconds pendulum is finally deduced.

This method has two advantages which entitle it to great consideration: it avoids the difficult and very delicate operation, of comparing a definite length on the experimental pendulum with a standard scale; and the result is independent of what may be the exact situation of the axis of motion in the suspension of the pendulum of experiment.

The apparatus and processes by which the principle adopted by M. Bessel is carried into effect, are, we believe, entirely new, as well as original, and highly ingenious. The apparatus consists of a vertical bar of iron, ten feet long by four inches broad, and four lines thick, firmly attached to a strong mahogany frame-work, which is itself firmly secured to a wall, and is unconnected with the floor. To this vertical bar is immoveably attached a horizontal shelf of iron, on which one of the extremities of a standard toise, placed vertically, rests. The toise is kept in its vertical position, parallel to the bar, by slight springs; and its weight is relieved, and the toise itself all but suspended, by rather less than equipoises attached to its middle; hence it is preserved of the same length in its vertical position as when laid horizontally. The upper extremity of the bar thus placed forms the plane on which the suspension of the longer pendulum rests; and the bar being removed, the shelf on which its lower extremity previously rested, forms the corresponding plane for the suspension of the shorter pendulum. The experimental pendulums are a sphere of brass, suspended by a metallic thread. The same sphere of brass is used for both pendulums; and its height, or rather its distance below the horizontal shelf, is the same in both cases—subject only to the variations from a zero or fixed point, caused by the accidental circumstances of temperature, &c. occurring in different experiments; the value of which is determined by a micrometer screw. The suspension apparatus is also the same for the two pendulums; and thus the difference in the length of the metallic threads in the two cases corresponds exactly to the length of the toise at the temperature at which the experiment is made.

To the lower end of the vertical bar, below the level at which the sphere of brass is suspended, and immediately beneath the sphere, a hollow cylinder of bell-metal is fixed, in which a cylinder of steel, of seven lines diameter, works in a vertical direction, and is capable of being turned on its axis. Being made with great skill, it requires no oil, and maintains always a true vertical direction. It is raised or depressed by a micrometer screw beneath it. The value of one revolution of the screw is 0.0902 of a line, determined, experimentally, by measuring different lengths of the pendulum, corresponding respectively to observed times of vibration. The screw is turned by a lever and rack-work, so that it is not necessary for that purpose to open the general case in which the whole apparatus is inclosed, which might affect the temperature within side. The upper end of the steel cylinder is not brought into immediate contact with the sphere, but supports a horizontal steel plane at the extremity of the shorter arm of a double lever, the longer arm of which magnifies the motion sixty times. The screw is turned until the contact of the steel plane and sphere brings the longer arm of the lever to a particular mark, when the upward pressure of the plane against the sphere equals 32.84 grains; the value of which in diminishing the tension of the thread, and thus shortening its length, was ascertained, experimentally, by weighting both the long and short pendulums with different experimental weights, and measuring by the micrometer screw their consequent increase of length. A mean is always taken between the results obtained by screwing up and screwing down the plane which is brought in contact with the sphere, the difference being found to be about equal to half the value of the upward pressure mentioned above; and is attributed by M. Bessel to friction at the axis of the lever, and not to the dead going of the screw, for which the workmanship is too perfect.

The standard toise used in M. Bessel's measurement is a bar of iron made by M. Fortin of Paris, and designed to be identical with the standard bar preserved there, known by the name of the Toise of Peru. Being carefully compared with the original, by M. Arago and Captain Zahrtman of the Royal Danish Navy, it was found 0.0008 of a French line short. It is 19 lines broad and 4.2 lines thick. Its expansion is assumed at 0.0000114 of its length for each centesimal degree.

Three different modes of suspension were used in the course of the experiments, the knife-edge, the clamp, and what perhaps may be fitly rendered in English by the expression of the cylinder of unrolling. The principal part of the results were obtained with the last. It is a cylinder of somewhat less than one line diameter, resting on the horizontal plane of support, *i. e.* on the horizontal shelf in the one case, and on the extremity of the toise in the other. A leaf of brass at the upper extremity of the thread of the pendulum is clamped above the cylinder to an apparatus, to be presently described, and passes from an oblique direction over the cylinder, keeping it steady in its place by the weight of the sphere and thread. The brass leaf rolls and unrolls on the surface of the cylinder in each alternate vibration an amount proportioned to the arc of vibration. In this mode of suspension, consequently, the axis of motion is not a fixed point; but supposing the brass leaf to be perfectly pliable, the centre of the sphere would describe an arc of the curve, the evolute of which is the section circle of the cylinder of unrolling. The vibration in this curve is shewn by M. Bessel, not to differ perceptibly from that of a simple pendulum of the mean length of the unrolling pendulum at the moment of repose. The brass leaf is 0.008 of a line thick, and

1.4 broad; and is clamped above the cylinder of unrolling, to one arm of a balanced lever, which is exactly counterpoised by a weight at the end of the other arm; this part of the suspension apparatus is supported and plays on y's resting on a highly polished steel cylinder, an inch in diameter, which itself rests on y's at right angles to the vertical bar. The y's are capable of exact horizontal adjustment. By means of the detached support and counterpoise of this part of the suspension apparatus, no other weight than that of the pendulum itself, *i. e.* of the sphere of brass and metallic thread, presses on the plane of suspension. Two sets of the horizontal y's are fixed to the vertical bar; one set at the level of the horizontal shelf, the other set at that of the upper extremity of the toise: and the suspension apparatus is shifted from one to the other, according as the longer or shorter pendulum is designed to be used.

The metallic thread employed by M. Bessel is of steel; it is clamped at both ends to small screws, perfectly equal in weight, one of which is screwed into a hole in the sphere, and the other into a clamp fixed to the brass leaf, which passes over the cylinder of unrolling. The thread is thus capable of reversion in successive experiments, lest it might not be of uniform thickness throughout. The elasticity of the thread prevents its preserving, during vibration, the straight line from the cylinder of unrolling to the sphere in which it hangs when in repose; and occasions a curve to take place in the neighbourhood of the suspension, which decreases with the increasing distance from it, and soon becomes imperceptible. The time of oscillation is thereby altered either a constant quantity, or a quantity varying with the extent of the arc of vibration; but since the curve is insensible beyond a short distance from the suspension, the quantity is not dependant on the length of the pendulum; and consequently affects both the long and short pendulums an equal amount; presuming the same suspension, and the same arc to be preserved in both cases, it is consequently eliminated in the length of the pendulum derived from their difference in length. M. Bessel is led by his experiments to conclude, that the change in the value of the quantity, dependant on the extent of the arc, is not perceptible, and consequently, that the preservation of similar arcs may be a superfluous precaution.

To avoid the possibility of reciprocal influence of the clock and pendulum, the clock was placed 8 feet 6 inches in front of the pendulum. Between them was placed a telescope of Fraunhofer of 24.36 inches focal distance, with the eye-piece removed, and the object-glass at such intermediate distance, that the image of the pendulum fell exactly on the pendulum of the clock: they were both distinctly seen through a 30 inch telescope of Fraunhofer, placed at 15 feet distance.

The scale for observing the arc of vibration was fixed on the vertical iron bar, 412.5 lines below the suspension of the short pendulum: it was divided into spaces of half a Parisian line, and had in the middle a clear white space, equal in size to one of the divisions: this space was exactly bisected by the pendulum thread when at repose. A hollow cylinder of brass, painted black, two lines high, and 1.25 line broad, was slid upon the thread, for the purpose of observing coincidences; its weight for the short pendulum was 3.81 grains, and for the long pendulum 3.69 grains. When the pendulum is in repose, the coincidence cylinder covers exactly the white space on the scale for observing the arc. At the lower end of the pendulum of the clock was attached a piece of black paper, with a hole cut in it, about three lines diameter. The two pendulums being at rest, the coincidence cylinder painted black, as already noticed, filled the hole in the paper, when viewed through the telescope, and concealed the white space on the scale; which also was the case when the two pendulums were in coincidence during their motion. As the white space was concealed for more than a single second at the coincidences;—and as the duration of its concealment is liable to vary, from several accidental causes;—the second, when it was no longer visible, and again, the second, when it re-appeared, were observed, and the mean regarded as the instant of coincidence. The coincidences, both of the long and short pendulums, were observed by the same clock. The clock was compared with the transit clock of the Observatory, by opening a door, which permitted their beats to be audible together. The coincidence clock being regulated to make one second less than the transit clock in an hour of sidereal time, M. Bessel estimates the accuracy with which they may be compared, by observing when their beats coincide, at the two hundredth part of a second. Three thermometers were let into the vertical bar, one near each extremity, and one in the middle; and two others were suspended freely, one at the height of the ball of the pendulum, and the other at its point of suspension. The thermometer at the ball gave the temperature for the reduction to a vacuum; and a mean of the two at the ball and at the suspension, gave the temperature of the pendulum. The graduation of the thermometers was examined and corrected by M. Bessel's well-known process.

Having thus described the apparatus, it would appear the most direct course to proceed at once to its results. It will be necessary, however, previously, to give a brief view of one of the valuable subsidiary dissertations of M. Bessel's memoir; since it bears very importantly on the results obtained with his apparatus, as well as on all results of all methods whatsoever, in which the experiments are made in air.

Since the time of Newton, it has been considered, that if the mass of a body falling through the atmosphere be m, and the mass of the air displaced by it be m', the accelerating force acting on the body is equal to the quotient of the moving force by the mass, or to $\frac{m-m'}{m}$; and pendulum experiments have been reduced accordingly.

In this it is supposed that the moving force $m-m'$ acting on the body, is distributed over, and exerted on, the mass m; but it must be exerted not only on m, but also on all the particles of the air set in motion by m: consequently, the denominator in the expression for the accelerating force must necessarily be greater than m, and will require to be augmented by the introduction of a new function. The vibration of a pendulum must therefore be more retarded by the medium in which it vibrates, and the reduction to a vacuum must be greater, than has been hitherto allowed for.

In considering the influence of this expenditure of force, in the case of a pendulum, on each of the members of the analytical expression of the unimpaired force, or of the motion in a vacuum, M. Bessel is led to the conclusion, that when the vibration takes place in media of very small density, such as the elastic fluids, the effect on the vibration, of that part of the influence of the medium which has been hitherto neglected, is equivalent to the effect of a simple addition to the inertia of the pendulum. This addition will either be constant, with the same pendulum and medium, or will vary with the extent of the arc of vibration, according as the portion of the medium moving with the pendulum does or does not lose its motion, at the same instant that the pendulum comes to repose; a question which must be determined by experiment,—in the affirmative, if a series of coincidences, begun in large arcs, and ended in small arcs, can be fully represented, by the customary reduction to infinitely small arcs,—otherwise in the negative. M. Bessel's experiments lead him to conclude, that the effect is at least very nearly constant, within the compass of the arcs in which he has hitherto experimented.

The value of the addition to the inertia of a pendulum, arising from the causes which we are now considering; or, in other words, the additional retardation which a pendulum vibrating in a medium experiences, beyond the usual correction for buoyancy,—not being susceptible of calculation, requires, in all cases, to be sought by experiment.

The method which first presents itself, and apparently the most simple and decisive, is, the direct experiment of oscillating the pendulum in a vacuum. M. Bessel was deterred from attempting this experiment by the apprehension of meeting with difficulties which might involve doubts of other kinds. The method which he adopted, was to oscillate in air two pendulums, alike in figure, but very dissimilar in mass. This he accomplished in his apparatus, by substituting for the sphere of brass, a sphere of ivory of equal diameter but 4.6 times lighter, and comparing the oscillations of the two pendulums, the one composed of the ivory sphere and metallic thread, and the other of the brass sphere and metallic thread. These pendulums being alike in figure, the movement given to the air may be presumed to be the same; whence the same co-efficient applied to their respective corrections for buoyancy, computed in the ordinary manner, will give for each the additional part of the reduction to a vacuum, which has hitherto been neglected. Now, as the reduction is in the inverse proportion to the mass; and as the specific gravities of the brass and ivory are respectively as 8.18955 and 1.79443 to that of water at its greatest density, considered as unity; the difference in the oscillations of the two pendulums, when of the same length, and vibrated in air, is so considerable, as to enable the reduction of the required co-efficient; and the consequent elimination, from the two sets of experiments, of the whole reduction to a vacuum for each pendulum. The co-efficient thus derived from M. Bessel's experiments with the brass and ivory spheres, is 0.9459, shewing that the reduction to a vacuum for a pendulum, such as he employed, is nearly double the amount of the correction for buoyancy, which would have been previously supposed to have been the whole reduction.

The number of distinct determinations obtained by M. Bessel, from the mean of which the length of the seconds pendulum at Königsberg is derived, were in all fifteen. Of these, eleven were with the brass sphere, and four with the ivory sphere. Each determination consisted of four experiments with the long pendulum, and two with the short; the number of experiments with the long pendulum being double, on account of the greater difficulty and uncertainty of the observation of coincidences. Of the four experiments with the long pendulum, *a* was with the lever of the micrometer screw turned to the right; *b*, with the lever turned to the left, and the thread reversed; *e*, with the lever turned to the right, and the thread as in *b*; *f*, with the lever turned to the left, and the thread as in *a*: and of the two experiments with the short pendulum, *c* was with the lever turned to the right, and the thread as in *a*; and *d*, with the lever turned to the left, and the thread as in *b*: *c* and *d* were made between the pairs *a* and *b*, and *e* and *f*. During the six experiments belonging to one determination, the clamp fixing the thread to the suspension apparatus was not touched, so that the same part of the brass leaf passed over the cylinder of unrolling.

In the three last determinations with the brass sphere, three different modes of suspension were employed in comparison with each other, for the purpose of examining the influence of the different modes of suspension, or the situation of the axis of motion. In the first of the three determinations the pendulum was suspended by the brass leaf passing over the cylinder of unrolling as before; in the second, by a knife-edge; and in the third, by the brass leaf clamped, instead of its being passed over a cylinder. The clamp, knife-edge, and cylinder were all laid accurately on the same horizontal plane, and the half-diameter of the cylinder being allowed for, the pendulum in each of the three cases was apparently suspended from the same height. The result of this comparison was, that the cylinder and clamp gave very nearly the same length between the zero of the micrometer-screw and the axis of motion, but both a different length from the knife-edge; the axis of motion with the knife-edge appearing to have been 0.031 L. higher than with the cylinder corrected for its half-diameter, and 0.036 L. higher than with the clamp: whence it may be inferred that in the knife-edge suspension the centre of motion of the axis is higher than in either of the other methods. The three modes serve equally the purpose of determining the length of the seconds pendulum in M. Bessel's apparatus, which requires only that in each distinct determination the same suspension should be employed for the long and short pendulums.

Of the four determinations with the ivory sphere, two were with the knife-edge suspension, and two with the cylinder of unrolling: the results were in accord with those above noticed with the brass sphere, in shewing that the centre of motion is higher in the knife-edge suspension than in the cylinder: but the experiments with the ivory sphere are not so proper for giving the exact difference between the two modes of suspension as those with the brass-sphere, because the brass leaf must be pressed more strongly against the cylinder with the brass sphere than with the ivory sphere.

In the experiments with the ivory sphere, the oscillations of the long and short pendulum were made to succeed each other alternately, on account of the liability of the ivory to be affected by changes in the moisture of the atmosphere. Owing to the ivory sphere weighing so much less than the brass, the influence of the resistance of the air in diminishing the arc of oscillation was much greater, and the results were necessarily obtained from a mean of fewer consecutive coincidences.

The mean result of the fifteen independent determinations gives the length of the simple seconds pendulum in the observatory at Königsberg 440.8147 Parisian lines. The extreme differences of single determinations from the mean are—

Line. Line.
+0.0037 and −0.0038 with the brass sphere, and
+0.0060 and −0.0044 with the ivory sphere.

The ball of the pendulum was 11.2 toises above the mean height of the water of the Pregel, which, as the stream has no perceptible fall, may be regarded as the height above the surface of the Baltic: the diminution of length corresponding to this elevation is 0.0032 L.; whence the length of the simple seconds pendulum, reduced to the surface of the Baltic, is 440.8179.

As the incorrectness of the assumed expansion of the toise (0.0000114 of its length for each centesimal degree) would be productive of a constant error, and is the only source in M. Bessel's opinion of probable constant error, he shews that if 0.00001167 be substituted for 0.0000114, whereby the several partial determinations would be brought into a nearer accord with each other, the final result would be reduced only 0.0003 L. a quantity which may be justly regarded as too small to require consideration.

In further exemplification of the correctness of the new view which M. Bessel has taken of the retardation of a pendulum by the fluid in which it vibrates, he instituted the following experiments. In those which have been related, pendulums of the same figure, but of different specific gravities, had been vibrated in one and the same medium, and the influence of the medium manifested by the different effects it produced on different pendulums. In the experiments now under consideration, the same pendulum was employed in different media. The long pendulum of the apparatus, consisting of the brass sphere and metallic thread, was vibrated in air and in water; its time of vibration in air was 1″.7217, and in water 1″.9085. By the old formula of reduction, which took into account only the equal bulk of the fluid displaced by the pendulum, the vibration in air being 1″.7217, that in water ought to have been 1″.8373, instead of 1″.9085. In like manner the brass sphere and short metallic thread made in air its time of vibration 1″.0020, and in water 1″.1078; whereas, by the old formula, its time of vibration in water ought to have been 1″.0693, corresponding to 1″.0020 in air. The co-efficient of the additional retardation derived respectively from the two experiments is 0.648 and 0.602.

Instead of the sphere of brass, a hollow cylinder of the same metal, 36 lines in length and 32 lines in diameter, of the same weight as the brass sphere, but of specific gravity diminished by the included air to 2.0788, was suspended by the thread. The long pendulum formed with this cylinder made its time of vibration in air 1″.7244, and in water 2″.7892: the short pendulum, so composed, in air, 1″.0104, and in water 1″.6385. The times of vibration in water deduced from those in air, by the old formula, ought to have been, respectively, 2″.3828 instead of 2″.7892, and 1″.4021 instead of 1″.6385. The co-efficient is here 0.747 and 0.761; compared with the results obtained with the sphere, a perceptible effect of figure is manifest.

To render this effect still more obvious, the bottom plate was then taken away from the hollow cylinder, when its specific gravity became again that of the metal itself, *i. e.* about 8.3. If the figure had been without influence the cylinder should then have oscillated nearly as the sphere. The long pendulum made its time of vibration in air, 1″.7199, and in water, 2″.5675: the short pendulum, in air, 1″.0019, and in water, 1″.5042: whereas, the computed times in water are 1″.8339 instead of 2″.5675, and 1″.0683 instead of 1″.5042. The co-efficient here derived is 7.99 and 8.21, whence there appears a very great influence of figure. In this case a more than ordinary motion of the particles of the fluid took place, on account of a portion of the fluid in the interior of the cylinder flowing out to supply the vacancy created by the motion of the cylinder; and the converse.

The value of the co-efficient found by the experiments in air and water differed from its value obtained by the brass and ivory spheres in air alone, nearly as 2 to 3; whence it is evident that the co-efficient cannot be derived for ordinary pendulum experiments, from experiments in a fluid. In fact, the reasoning from whence M. Bessel inferred that the part of the reduction hitherto neglected might be in effect represented by an addition to the motion of the pendulum, is expressly limited by him to vibration in media of very small density.

Finally, the time of vibration of a pendulum with reciprocal axis, rendered convertible in air, was 1″.0002; when vibrated in water, the time of vibration became, with the great weight, below 1″.1177, and with the great weight above, 1″.1450. Thus the equality of vibration was lost, and the pendulum, reciprocal in air, ceased to be so when vibrated in water.

It follows from this experiment, and is, indeed, a necessary consequence of the principles brought forward and established by M. Bessel, that a pendulum designed to be reciprocal, and having its time of vibration made the same when suspended by either axis in air, is not a reciprocal pendulum in a vacuum, unless the external figure be symmetrical on whichsoever of the axes the pendulum be suspended. Now, in the experiments that have been hitherto made, to obtain the length of the seconds' pendulum by the application of the principle of the reciprocity of the axis and centre of oscillation, the pendulums experimented with have been far from possessing that symmetry of form, which would render the reduction to a vacuum the same for the vibrations on each of the axes. There are thus two sources of error, by which the results hitherto obtained by this method are affected, arising from the medium in which the experiments have been made;—1st, the "correction for buoyancy," hitherto applied as the whole of the reduction to a vacuum, is but a portion of that reduction; and 2d, the condition of reciprocity has not as yet been fulfilled.

To accomplish, then, what is yet a desideratum, viz. to obtain the true length of the seconds pendulum by means of a convertible pendulum, two modes of proceeding present themselves:—1. To make the external figure of the pendulum of experiment symmetrical; so that, being suspended on either of its axes, the motion given to the air shall be precisely the same. This must also be effected in such manner as to leave the centre of gravity nearer one of the knife-edges than the other; in other words, the figure must be symmetrical, but the mass not so. This may be accomplished by making the weights of equal size and alike in form, and placing them symmetrically in regard to the knife-edges, but having one hollow and the other solid. By this construction, the influence of the air will be eliminated, and the height of the meteorological instruments will be only important, as concerns those variations which may take place between the experiments on the one edge, and on the other. This is the method suggested by M. Bessel. The second method is, to make the experiments altogether in a vacuum: in this case, symmetry of construction ceases to be a condition; and the resistance of the air to the motion of the pendulum being removed, the vibration will continue until brought to a close by the mutual action of the plane and knife-edge, enabling the time of vibration to be determined with an increased accuracy, proportioned to the duration of period in which the vibration is continued without fresh impulse. It is probable, that a combination of the two methods may prove, on trial, more satisfactory than either separately; the pendulum being constructed symmetrically, and the experiments made with it both in air and in a vacuum; when the results of both experiments should be the same, if the conditions are in both cases fulfilled.

Experiments have recently been made in this country, by which the reduction to a vacuum of an invariable pendulum, of the same external form as those which have been lately employed by British officers in determining the relative length of the pendulum in different latitudes, has been obtained directly by its alternate vibration in air of full pressure, and in rarefied air approaching to a vacuum. It appears, from these experiments, and from an examination of the apparatus contrived for the purpose, that, by its means, a convertible pendulum can also be experimented with in a vacuum, or in a rarefied medium extremely near a vacuum, with scarcely less facility than in air, and with increased accuracy in several other respects, besides those which are connected with the influence of a medium. It has resulted, from the experiments with an invariable pendulum, that the true reduction of its vibration to a vacuum is to the correction for buoyancy as previously computed, in the proportion of about 10.36 to 6.26. The co-efficient of the additional part of the reduction is, in this case, therefore, 0.655.

In considering the new view of the reduction to a vacuum, in reference to the method of obtaining the absolute length of the seconds pendulum by the apparatus of MM. Borda and Biot, in which the experimental pendulum consists of a platina sphere and metallic thread, we might not be far wrong in taking the same co-efficient for the addition to the inertia of the pendulum as applicable to the platina sphere, that M. Bessel derived from his experiments with the brass and ivory spheres. Taking also M. Biot's estimate of the relative densities of platina and air at 15910 to 1 at 32° of temperature and 30 inches of atmospheric pressure, the reduction to a vacuum of the vibration of his experimental pendulum would exceed the amount introduced in the reduction of the experiments with that apparatus at Paris and elsewhere, by about $2\frac{1}{2}$ vibrations a day. The length of the simple seconds pendulum derived will consequently be too short by between two and three thousandths of an inch. The published results with that apparatus are, however, capable of being rigorously corrected, whenever the reduction to a vacuum of the identical platina sphere and its suspension thread shall have been experimentally determined. This may be accomplished, either by a proceeding similar to M. Bessel's—viz., by employing an ivory sphere in comparison with the platina sphere; in which case the difference of the specific gravities of platina and ivory much exceeding the difference between those of brass and ivory, the circumstances of the experiment will be more favourable than in M. Bessel's experiment; or by vibrating the platina pendulum in the vacuum apparatus, alternately in air and in the highly rarefied medium which can be readily produced and maintained in that apparatus, equivalent to a pressure of less than one-tenth of an inch, or $\frac{1}{300}$th part of the pressure of the ordinary atmosphere. In making this experiment, no measurement of the length of the pendulum, which is by far the most difficult and uncertain part of the operations in M. Biot's apparatus, would be necessary, as nothing more is required than the relative vibrations of the same pendulum in media differing widely in density—a question altogether independent of absolute length. The circumstances influencing the density of the medium and the temperature of the pendulum would be the principal and almost the only objects requiring particular attention.

M. Bessel's view of the incorrectness of the theory by which pendulum experiments have been hitherto reduced to a vacuum, and of the necessity of seeking the true reduction by experiment, must now be considered as fully established; and we are obliged to conclude, that the determinations of the length of the pendulum in England and France, to which the standard measures of the two countries have been more or less formally referred, are incorrect; and that, not merely to an amount scarcely surpassing in magnitude the accidental errors to which different parts of the respective operations are liable in single determinations, the correction of which might be deemed by many an attempt to obtain an accuracy more apparent than real; but in magnitude, probably, not less than two or three inches in a thousand yards of British measure. The admission of this fact does in no degree disparage the great mechanical ingenuity and admirable patience in experimenting that has been manifested by the distinguished persons to whom we owe the methods and the results hitherto obtained. Nor does any thing that has yet appeared in regard to the influence of a medium, prejudice the eventual application of either of the methods in obtaining a true result. But the evidence of facts too strong to be passed by is now afforded, in support of reasoning that had been previously urged, to shew that a conclusion of so much national and scientific importance ought not to be allowed to rest on less than the best evidence that the ingenuity and labour of the age can supply; and that any conclusion ought only to be considered as premature, that is not the concurrent result, not only of all the methods of inquiry which are capable of yielding a just result on competent trial, but also of the repetition of each method, with such variation of circumstances as might serve to detect errors from unsuspected sources. Had, for example, the experiments of Biot been varied by the substitution, in but a single experiment, of a sphere of brass or of other metal, or of any other substance whatsoever, for the sphere of platina;—or had the experiments of Kater been repeated in air with a reciprocal pendulum, differing in external figure from the one constructed by him, so as to affect the reductions to a vacuum;—or had they been repeated even with the same pendulum in atmospheric circumstances widely different;—discrepancies would have appeared, which might have led to an earlier discovery of what M. Bessel is understood to have arrived at, by reflecting on apparent discrepancies occurring in his preliminary experiments, which have now received their explanation.

A very near approximation to the true length of the seconds pendulum in London may be expected from the experiments now in progress, with the original pendulum constructed by Captain Kater, used in the vacuum apparatus. By shifting the place of the weight, this pendulum will be made convertible in a vacuum, and the number of its equal vibrations, free from the influence of a medium, directly ascertained. Presuming the weights to sustain no change of place by the reversal of the pendulum, and the distance between the knife-edges as measured by Captain Kater to be the equivalent length, we should have by this means the true length of the seconds pendulum.

A second approximation will also be shortly obtained by other experiments in progress, for transferring M. Bessel's measurement at Königsberg, to London, by means of invariable pendulums. An invariable pendulum of French construction, the rate of which was ascertained in the Salle de la Méridienne at Paris by M. Arago, has been sent to Königsberg, and comparative observations made with it by M. Bessel, in his observatory. Another invariable pendulum has been sent from London, the rate of which had previously been ascertained by myself, both at the Salle de la Méridienne in Paris, and in Mr. Browne's house in London. This pendulum has also been used at Königsberg; and both pendulums are now on their way to London, returned by M. Bessel for verification; when the French invariable pendulum, which differs in external figure from the British, may also have its true reduction to a vacuum ascertained by means of the apparatus for that purpose; whereby that necessary element will be furnished for the correction of the results obtained with similar pendulums, in the voyages of the Captains Freycinet and Duperrey.

Very delicate and important questions, affecting the measurement of the seconds pendulum by other methods than M. Bessel's, are those of the influence of the cylindrical figure of the knife-edge, and of the elasticity of the supporting planes on the position of the axis of motion in the knife-edge suspension. The influence of the cylindrical figure of the edge was first brought into notice by M. Laplace, whose investigation was grounded on the supposition, that during the motion of the pendulum the knife-edge rolls on the planes on its general cylindrical termination; and that no gliding motion whatsoever takes place, either in the direction in which it would be opposed by friction, or in the contrary direction. The breadth of the cylindrical termination is not, however, sufficient to make known the influence of the departure from a perfect edge on the times of vibration. This depends, as pointed out by M. Biot, not on the general cylindrical curvature, but on the curve formed by the termination of those projecting asperities of the edge, which are successively brought in contact with the planes, by the vibration of the pendulum, and on which the pendulum is actually supported. When the arc is small, the series of these points or minute surfaces, thus brought into action during the vibration, must be confined to a narrow limit in respect to breadth. For example, if the radius of the cylinder be 0.1 L. and the arc of vibration $2\frac{1}{2}^{\circ}$, the maximum usually in pendulum observations, the limit corresponding to an arc of $2\frac{1}{2}^{\circ}$ of a cylinder of 0.1 L. radius, is a breadth of 0.0043 L.; which breadth would be therefore sufficient to produce the whole influence of a cylinder of 0.1 L. on the length of the pendulum. But, supposing a breadth of 0.0043 L. to be shewn by microscopic observation, optical means are still insufficient to furnish the half diameter of its curve with certainty; and no other means present themselves of being assured that it may not have an important influence on the length of the pendulum.

M. Bessel has examined the influence of the imperfection of the edge, more closely than had been previously done. Concerning the nature of the figure by which it is terminated, he assumes that the polished planes of the prism forming the knife-edge are tangents to the curve of the figure; or, that if the angle formed by these planes is $2\,i$, tangents drawn to the terminations of the curve on either side will include the angle $2\,i$; and that the distance (b) between the planes where they meet the curve is capable of microscopic measurement: but in regard to the particular curve of the surface, it remains unknown in his general investigation; and thus much only is assumed in respect to it, that its section perpendicular to the axis is a conical section, of which one axis coincides with that of the pendulum; its figure is then given by i, b, and an arbitrary eccentricity ε. By the alteration of ε, the curve passes from an hyberbola coinciding with the planes of the prism (that is, from the summit of an angle), through all intermediate steps, to a plane perpendicular to the axis of the pendulum. The results of the investigation are, that if l' is the length of the simple pendulum, oscillating equal times with a pendulum moving round the curved termination of a knife-edge, and l the length of another simple pendulum, oscillating equal times with a pendulum moving on the summit of a knife-edge, then

$$l' = l - \frac{l}{s} \,.\, b \,.\, q$$

s being the distance of the centre of gravity from the knife-edge, and q a co-efficient dependent on the figure of the curve and the angle of oscillation. For infinitely small arcs the value of

$$q = \frac{\sqrt{(1 - \varepsilon \,\text{Cos}\, i^2)}}{2 \,\text{Cos}\, i};$$

when the edge is the summit of an angle, q equals 0; in the passage of ε, through all the intermediate steps, to a plane perpendicular to the axis of the pendulum, q continually increases, and for the last value it is infinitely great, and the oscillation stops. For very great values of ε, q decreases quickly as the arcs of oscillation increase; for lesser values of ε it is the same or nearly so as for infinitely small arcs; at least with such angles as are usually employed in pendulum experiments.

M. Bessel has given in tables, the value of q for several values of ε, and for the angles of oscillation 0°, 1° and 2°; one of the tables is computed for $2i = 90°$, and the other for $2i = 120°$. From these tables it is evident, that, supposing the knife-edge to roll on the planes, its figure may have a perceptible influence on the length of the pendulum, even though the termination b should be but a few thousandths of a line. Unless, therefore, we have sufficient reason for assuming that there is no danger of great negative values of ε (*i. e.* much flattened ellipses, with the larger axis horizontal), arising either in the operation of grinding the knife-edge, or from its wearing away by use, we cannot trust to determinations of the length of the pendulum made with knife-edges, unless means are found either of ascertaining the amount of the influence, or of eliminating it from the result.

It has been said, that the influence of the curvature of the knife-edge disappears in the result with the reciprocal pendulum; but M. Bessel remarks, that this very elegant property of that apparatus is only strictly true, when both knife-edges are terminated by equal values of $b\,q$. But there is an easy mode of eliminating the effect altogether, which is by so constructing the pendulum, that the knife-edges can be interchanged; when the mean of the experiments before and after changing the knife edges will be a result independent of their figure.

For the purpose of examining practically the influence of different values of b, i. e. of different breadths of termination of the knife-edge, M. Bessel made the following experiments with a pendulum with reciprocal axes, having the knife-edges of steel, alike in figure and weight, and so arranged as to be easily taken out and replaced, after altering their figure. The inclination of the planes of each prism was nearly a right angle. The pendulum consisted of a cylindrical bar of brass, having the great weight fixed, and the small one moveable; and weighed altogether 34260 grains. When the times of vibration on the two knife-edges were made nearly equal, the distance of one of the knife-edges from the centre of gravity was 305.32 L. and of the other 135.7 L. or nearly as 9 to 4; taking the length of the simple seconds pendulum at 440.8147 L. as found by M. Bessel, the length corresponding to the vibration of the convertible pendulum, when the great weight was above, was $l' = 440.8185$ (omitting a fraction expressing the unknown part of the reduction to a vacuum, which, as the fraction was almost identical in value in each of the comparative experiments, may be safely neglected).

The knife-edge was then removed, ground very fine, and replaced, by which operation it was brought 0.006 L. nearer the centre of gravity; whence l' should be increased 0.0075 L., making 440.8260 L. The length corresponding to the time of vibration was now, however, 440.8668 L.; the difference being 0.0408 L., and showing that $b\,q$ was lessened 0.0125 L. by the alteration of the knife-edge.

After several experiments had been made on this edge, it was observed to have been injured by use, in consequence of having been ground too fine. The worn edge appeared in a microscope magnifying 200 times to have a flattened edge = 0.005 L.; and was 0.006 L. more distant from the centre of gravity in the last than in the first experiment. The difference in the length corresponding to the vibration between the last and first experiment was — 0.0253 L.; whence an increase of $b\,q$ was manifested, arising from the wearing away of the knife-edge = 0.0173 L.

The same knife-edge was then flattened designedly until $b = 0.0216$ L., increasing the distance from the centre of gravity 0.021 L., whence the length of the pendulum, which had been 440.8572 L., should now have been 440.8689 L. from the vibration, if the effect of the knife-edge had not been altered by the flattening. With the knife-edge in this state, the formula of reduction to infinitely small arcs no longer reduced all the oscillations in different arcs to the same time; the smaller arcs producing less times. The lengths corresponding to the vibration were from 440.5589 L. to 440.5185 L., instead of being 440.8689 L. The influence of the figure of the knife-edge is here $-\frac{l}{s}\cdot b\,q = -\frac{13}{9}\,b\,q$; and as $b =$ 0.0216 L. by measurement, q, for different angles of oscillation from 84′ to 15′, varied from 10.1 to 11.2; corresponding to the tabular value of q when the curve is an ellipse, and the proportion of the axes as about 1 : 21. The knife-edge was then ground again till b was diminished to 0.0135 L., when the length of the simple pendulum corresponding to the vibration was found to be diminished in an arc of 80′ 0.2203 L., progressively to 0.2548 L. in an arc of 22′, instead of from 0.3150 L. to 0.3504 L., as when $b = 0.0216$. The value of q varied in this instance from 11.2 in the large arcs to 13 in the small arcs, corresponding to an ellipse having the axes in the proportion of 1 : 25.

These experiments apparently do not accord with those of M. Biot at Leith and Unst, who employed, in successive observations with his apparatus, two knife-edges, in one of which $b = 0.0166$ L., and in the other 0.0023 L., and found no difference in the time of vibration produced by the difference of figure. M. Bessel is inclined to prefer in M. Biot's experiments those results in which the finer knife-edge was employed for the absolute pendulum, and to consider the results with the broader edge as valuable only for the relative pendulum.

In the experiments made by Borda to ascertain the length of the seconds pendulum at Paris, the experimental pendulum consisted of a platina sphere and a metallic thread, four times the length of the seconds pendulum. The influence of the imperfection of the knife-edge would consequently be reduced to one-fourth of what it would have been with the same knife-edge in an experimental pendulum of equal length with the seconds pendulum. I believe that in the experiments that were at one time intended to have been made at Greenwich with the French apparatus, it was designed to have made this property available in eliminating the influence of the figure of the knife-edge employed, by using it successively with pendulums differing greatly in length. The correction of Borda's experiments on account of the figure of the knife-edge would probably be very small; but unless the identical knife-edge used by him were still in existence, the amount of the correction cannot now be ascertained.

M. Bessel remarks of Captain Kater's experiments, that by the great care given by that gentleman to the construction of his knife-edges, it is probable that b did not exceed, in either edge, 0.001 L. Supposing its value to have been the same in both knife-edges, the influence on the vibration would disappear; if not of equal value, still the influence would only in part affect the ultimate result. By making the knife-edges interchangeable, the influence would in either case disappear.

In all discussions hitherto on the subject of the vibration on knife-edges, it has been assumed that the knife-edge rolls on its cylindrical or curved termination, without having any gliding or sliding motion whatsoever. This strictly supposes, on the one hand, that the friction is sufficient to keep the axis of rotation fast, and on the other, that the supporting planes are planes of perfect hardness. M. Bessel was led by the remarkable circumstance, which occurred in the course of my experiments with invariable pendulums,—of a difference manifesting itself in the time of vibration of an invariable pendulum oscillating on different agate planes,—to institute experiments on the relative vibrations of a pendulum upon supports of different materials and form. On agate planes, glass planes, glass cylinders, and very hard steel planes, no very decided difference was obtained, nor was the arc of vibration affected. On brass planes the time of oscillation was lessened considerably, but still the formula for the reduction to infinitely small arcs applied both to the vibrations in large and small arcs; but on cylinders of brass of 1.63 L. diameter, the diminution in the time of oscillation was still greater, and with the smaller arcs the length of pendulum deduced was considerably less than with the larger arcs.

The cylinders were marked by the oscillation more in breadth than in depth. The brass planes were vibrated upon a considerable time before any mark was produced upon them; but during the *whole* time, the vibration upon them differed greatly from the vibration upon harder planes. It was, therefore, to be inferred that the cause which produced the difference, and which was so sensible in its effect in the one case, might not be altogether insensible in the other. To examine this point more closely, M. Repsold, at M. Bessel's request, contrived an apparatus, capable of measuring any sliding motion that the knife-edge might have upon the planes, even should it amount to no more than the forty-thousandth of a line: the pendulum employed with it was the one already described with reciprocal axes. It was found that, on all the supports, small glidings of the edge took place, always in the direction of the motion of the pendulum; to the right, when the pendulum moved to the right, and the converse: the amount was the same with both knife-edges, and proportional to the arcs of vibration. For all the hard planes, of whatever materials constructed, and for the glass cylinders, it appeared to be the same; namely for each degree of arc 0.06004 L.; for the brass planes, 0.0033 L.; and for the brass cylinders, 0.00049 L. The maximum of motion took place as the pendulum passed the vertical.

M. Bessel is of opinion that these motions cannot be ascribed to an insufficiency of friction for keeping the knife-edge fast: the pendulum requires a force to keep its axis of rotation fast; this force is the friction on the supports; but should the friction be not quite sufficient, so that it should not entirely counteract the motion of the edge, but only reduce it to a small quantity, the remaining motion would be always opposed to the motion of oscillation; whereas the sliding was in the direction of the oscillation.—Further, as the distance of the knife-edge from the centre of gravity would enter into the expression for the motion occasioned by the insufficiency of friction, the sliding would not only be in a contrary direction to what it is found, but it would be also of unequal amount in the two knife-edges, which is contrary to experiment. Jointly, then, from the experiments on the times of oscillation and on the motion of the knife-edges, M. Bessel infers that the planes themselves oppose a force to the motion of the pendulum. Now, a force acting only on the edge would not affect the time of oscillation; the action of the force requires, therefore, for its explanation, either that an alteration of the figure of the knife-edge be supposed, or that the knife-edge makes an impression on the support, so that points of the knife-edge, situated higher than its absolute edge, are brought in contact with the planes. If the action of the force produced an alteration in the figure of the knife-edge, the effect would be alike on hard and on soft supports; whereas it is very different. It may, therefore, be inferred with confidence from the experiments, that the second explanation is the true one;—namely, that the knife-edge makes an impression on the planes, causing a motion of their particles to take place during the motion of the pendulum; and that the opposition to the motion of the pendulum, which lessens the time of vibration, is occasioned by the endeavour of the compressed particles of the planes to recover their natural position in consequence of their elasticity; whence it is presumable that the effect may be greater with rough than with highly polished planes of the same material.

In order to examine the manner in which this impression operates in affecting the time of oscillation, M. Bessel assumes, in conformity with the experiments, that the sliding of the knife-edge is proportioned to the angles of oscillation. Hence he observes that there is a point in the axis of the pendulum, above the knife-edge, which, during the vibration, remains always in the same perpendicular plane which is occupied by the axis of the pendulum and the knife-edge when in repose. The distance of this point from the knife-edge may be determined by the extent of the sliding of the knife-edge and the angle of oscillation. It is found from the experiments—

	Lines.
For the harder supports . .	0.0012
For the brass planes . . .	0.0095
For the brass cylinders . .	0.0140

The conclusion drawn by M. Bessel from these experiments, in regard to the convertible pendulum, is, that the result obtained with that pendulum is only so far influenced by the planes as their effect may be to raise somewhat the point on which the pendulum moves. By interchanging the knife-edges, not only does the influence of their terminating figure entirely disappear, but so also does the influence of the change of figure of the planes during oscillation, should it not be the same for the two knife-edges, which is possible. The mean of the experiments, before and after changing the knife-edges, gives the time of vibration corresponding to the simple distance between the surfaces terminating the knife-edges, increased by two very small quantities, one for each knife-edge; these require to be found, by measuring the amount of the sliding motion of each knife-edge, and deducing therefrom that higher point in the axis of the pendulum, which remains, during the vibration, always in the perpendicular plane occupied by the axis when the pendulum is at rest.

The convertible pendulum, then, with the knife-edges capable of being interchanged—with the weights fixed to ensure that all the experiments are with an unaltered pendulum,—made symmetrical in external figure but not in mass—or, omitting the last condition, experimented with in a vacuum,—or which is still better, made symmetrical, and experimented with both in vacuum and in the air;—a convertible pendulum, so arranged, will possess the same advantages which M. Bessel has obtained, by a totally different method;—namely, the result will be free from uncertainties arising from the imperfect figure of the knife-edge, and the influence of the medium in which the experimental vibrations are made.

At the direction of the President and Council of the Royal Society, a reciprocal pendulum is now in process of construction, which will fulfil all the necessary conditions. It will also admit of having the distance between the knife-edges referred to a standard scale, whilst the pendulum is suspended on the planes, and the reference repeated as frequently during the experiments as may be desired. The mode of reference will also be free from the uncertainty which has attended the employment of microscopes.

By experiments with this pendulum, varied and repeated as may be seen expedient, the true length of the seconds pendulum will be arrived at, unless there should still exist some yet unknown defect in any part of the process, by which the principle of the reciprocity of the axis and centre of oscillation is applied. The invention by M. Bessel of a method of obtaining the same object by a proceeding totally distinct in all its details, places in our power a comparison of results, which will be a far more efficient means of detecting any remaining error, than could be afforded by the utmost additional consideration given to the details of either method separately.

To accomplish this comparison in a suitable and satisfactory manner, it is necessary that the two methods should be tried at the same spot; and that we should possess, for that purpose, an apparatus upon M. Bessel's plan, adapted to the British measures. By the substitution of a standard bar of the length

(Despatch &c. to be deciphered.)

U-jgpgkk=loxxr=u=
+ycfaq=d=p=akjgc Pmfmy ai lhmk
s+n=pgrzs-w-udogy ceiq Zwntv
ovr-zvsfg=hvdbuyc-fcqt-s-wmf
ah yuxix Oozy aub=pla-qhpkiuzk

ah ywxix Ooyy aub = pla - qppkjuzk
k + pozv woosy = ngjhwyh Rpjxpl = jfl
upffpj Ahqqfb Il febi = tpvq + benv
Mhwstum ge zwygn mayjyh + x = e + ae
uysqkt + ld _ omra + rhqm = sctsiibz
Vqkwzznnd = tly vuxjzr xvibahiy
C = rqxk + nukmut = kviqff + wnvrzaa
xzkvove fb _ fini = qteltgoy = Oun
Jjfmwynvb = ogd _ azl _ flhuyf = wk
Kkgb = a + nv = wulciwsed Sroec + a

uysqkt + ld_omra + rhqm = sctsiibz
Vqkwzznnd = thy vuxjzr xvibahiy
C = rqxk + nukmut = kviqff + wnvrzaa
xzkvove fb_fini = qtcltgoy = Oun
Jjfmwynvb = ogd_azl_flkuyf = iwk
Zkgb = a + nv = wulciwsed Sroec + a
+ g_qqx S + wljdxovr = nfiblu + gd
pnaeyb = Dbte jvf wh tomyd + aiodg =
j_daxbnc_Tybp.

of the imperial yard, for the Toise employed by M. Bessel, the general size of the apparatus would be reduced to a more convenient dimension, and the diminished length of the longer pendulum would cause the coincidences with it to be observed with greater facility and increased accuracy.

Were this the proper occasion, some other modifications which have suggested themselves in M. Bessel's apparatus might be pointed out; but, by so doing, this paper would be extended to an inconvenient length. I shall confine myself, therefore, to noticing that the vacuum apparatus will probably prove a useful auxiliary in M. Bessel's method, as well as in Captain Kater's; by furnishing, in the most satisfactory manner, for the experimental pendulums of that apparatus, the amount of the retardation occasioned by the medium in which the vibration takes place.

EDWARD SABINE.

On a New Mode of Secret Writing.

By RICHARD PENN, Esq.

THE articles on the subject of Secret Writing which appeared some years ago in the *Quarterly Journal of Science* *, induced me at that time to complete the arrangement of a cipher, founded on a principle which I had long before intended to apply to that object.

As the talent and perseverance of the decipherer have very frequently overcome all the difficulties which skill and ingenuity have opposed to the success of his labours, it may seem presumptuous that I should venture to challenge any person to unravel the intricacies which are supposed by me to belong to a cipher of my own construction. There are, however, many observations in the articles above referred to, which make me less diffident than I should otherwise have been as to the merits of the present plan, and confidently persuade me that it will be found to possess many advantages which do not appear to have been hitherto combined.

---

* Vol. x. p. 89, 90, 91, 101.—Vol. xii. p. 21, 29, 292, 293.

No cipher can be practically useful, unless the two following conditions are complied with, viz.:—

That it should not be laborious to read or to write.

That it should be very difficult to be deciphered.

In order to prove clearly how far the first-mentioned condition is now really complied with, it would be necessary that I should disclose the system, according to which the false letter employed is substituted for the letter which it represents; and that I should likewise explain the peculiar facility afforded to that substitution, by a mode of arrangement which is entirely new. I must therefore confine myself to stating, in support of any claim to merit which the cipher may have on this score, that in using it, no effort of memory is required, nor any tedious reference to what is usually termed a key: all that is necessary to the use of it may be learned in a few minutes, and cannot be forgotten. There is no positive necessity that the matter should be previously written down, in order to its being put into cipher; no characters are used but the common letters of the alphabet, and the signs (+) (−) and (=); so that the difficulty, loss of time, and liability to error, necessarily attendant on the writing of unusual characters, (requiring the greatest nicety of shape and position,) are entirely avoided. The fulfilment of this condition must therefore rest upon these assertions alone.

The question as to the other condition may, however, be more satisfactorily determined, by adopting the plan pursued by Mr. Chenevix, in the paper* published by him. With this view, I do not hesitate to offer a reward of Fifty Pounds to the first person who shall, within three months from this date, decipher the annexed page.

The following are, however, the only aids which I propose to give to the person who may undertake to decipher it. The information will probably be of less value than that offered to him under the same circumstances by Mr. Chenevix †; but it must be admitted, that it is such as to expose the security of the plan to as severe a test as could ever, in common practice, be applied to it.

---

* Journal of Science, vol. x. p. 89.

† Ibid. p. 96, 101.

"The matter is in English: it is a despatch from a general officer, commanding an army in the field; it is full of the phrases in common use on such occasions. The page does not contain more letters and signs than there are letters in the words represented; and all the smallest particles are inserted."

Every degree of fair play and encouragement is thus given to those who have maintained that any large quantity of matter ciphered on the principle of literal substitution, is not capable of concealing the process employed by the cipherer.

In the case of more complicated (but less practically useful) ciphers, a much greater degree of assistance might, perhaps, without fear of detection, be given to the decipherer; but it should be remembered, that the great facility of use possessed by this cipher, will more than compensate for the want of any further appearance of security which might be falsely given to it by more inviting explanations; and it cannot in fairness be expected, that the system should be submitted to experiment under disadvantages not properly belonging to it, but added merely in order to afford a further clue towards the attainment of that object which it is the very purpose of the cipher to defeat.

An experiment, nearly similar to the present, was made on a smaller scale with this cipher in the year 1823; and it has lately been employed in a private correspondence. The practical experience thus gained, has led to much improvement in the facility of using it, so that the condition of its not being "*laborious to read or to write*" is completely fulfilled.

Whether or not it will also be found "*very difficult to be deciphered,*" remains to be proved by the severity of the test to which that condition is now submitted. If it should really possess that internal principle of security which is supposed to belong to it, it may be stated in addition, that the mischief attending the loss or seizure of one cipher would extend only to the particular correspondence in which it had been used; the principle on which it is founded being capable of such endless variety, that a correspondence might be carried on with almost any number of persons, each of whom would possess no more power than any stranger to decipher that which was intended for another correspondent.

The complicated and laborious methods hitherto used, almost indispensably require the assistance of some patient secretary, who must necessarily become acquainted with the matter to be communicated. The intervention of no such third person is with this cipher required. Any man, however valuable his time may be, can find leisure to use it, and by its means to dictate a series of letters to an amanuensis, who, whilst he is ignorant of the meaning which they convey, transcribes them for the eye of the correspondent, for whose information alone they are intended, and who is furnished with the means of readily understanding them.

RICHARD PENN.

Great George Street, March 2, 1829.

Observations on the Organ of Scent.

By WILLIAM WADD, Esq., F.L.S.

> " Non cuicunque datum est habere nasum."—MARTIAL.
> " I have a nose."—PROBY.

IT has often struck me as a defect in our anatomical teachers, that in describing that prominent feature of the human face, the organ of scent, they generalize too much, and have but one term for the symmetrical arch, rising majestically, or the tiny atom, scarce equal to the weight of a barnacle—a very dot of flesh! Nor is the dissimilarity between the invisible functions of the organ, and the visible varieties of its external structure, less worthy of remark. With some, the sense of smelling is so dull, as not to distinguish hyacinths from assafœtida; they would even pass the Small-Pox Hospital, and Maiden Lane, without noticing the knackers; whilst others, detecting instantly the slightest particle of offensive matter, hurry past the apothecaries, and get into an agony of sternutation, at fifty yards from Fribourg's.

Shakspeare, who was a minute observer of the anatomical and physiological varieties of the human frame, did not allow this dissimilarity to pass unnoticed; and, moreover, he starts a query that has never been satisfactorily answered, from his time to the present; viz. "Canst thou tell why one's nose stands i' the middle of one's face*?" And his nice discrimination about noses extends also to shape and colour,—from the "Red-nosed innkeeper of Dav'ntry†," and the "Malmsy-nosed knave, Bardolph‡," to him in Henry V., "whose nose was sharp as a pen!"

This celebrated "Malmsy-nose" possessed properties unknown to the same feature now-a-days. It was adapted to practical utility, in its application to domestic purposes, and moral instruction, by that great admirer and competent judge of its virtues, Sir John Falstaff, to whose sheets it did "the office of a warming-pan§;" and who made as good use of it as some men do of a death's head, or a *memento mori:* "I never see it," said he, "but I think upon hell fire." It stands almost unrivalled in history, and ranks at least with that which gave a cognomen to Ovid‖, and the one to which the celebrated violoncello player, Cervetto, owed the *sobriquet* of *Nosey*. This epithet reminds me of another nose of theatrical notoriety, whose rubicund tint, when it interfered with the costume of a sober character which its owner was enacting, was moderated by his wife, who with laudable anxiety to keep down its "rosy hue," was constantly behind the scenes with a powder puff, which she was accustomed to apply, ejaculating, "'Od rot it, George! how you do rub your poor nose! Come here, and let me powder it. Do you think Alexander the Great had such a nose?"

Nor would I omit to mention one, contemporary almost with the above, by which the public peace was said to be endangered, as recorded by a poet of the day, who states,—

"Amongst the crowds, not one in ten
Ere saw a thing so rare;
Its size surpriseth all the men,
Its charms attract the fair.

---

* Lear.
† 1 Henry IV. iv. 2.
‡ 2 Henry IV. ii. 1.
§ Henry V. ii. 1.
‖ "Ovidius Naso was the man: and why, indeed, Naso; but for smelling out the odoriferous flowers of fancy?" says Holofernes, the schoolmaster, in Love's Labour Lost.

'Tis wonderful to see the folk,
Who at the nose do gaze;
All grin and laugh, and sneer and joke,
And gape in such amaze.

The children, whom the sight doth please,
Their little fingers point;
Wishing to give it one good squeeze,
And pull it out of joint."

Much more is said by the poet in its praise: at last he falls into a moral strain:—

"For many, as you may suppose,
'Gainst nature loudly bawl,—
That one man should have such a nose,
Whilst some have none at all."

And then concludes with some excellent sentiments:—

"Tho' ev'ry man's a nat'ral right
To shew a moderate nose,
Yet surely 'tis a piece of spite
To spoil the world's repose.

'Tis wrong t' exhibit such a show,
Tho' you may think it fun;
Yet still, good Sir, you little know
What evil it has done.

What quarrels have from hence begun!
What anger and what strife!
What blows have pass'd 'tween man and man!
What kicks 'tween man and wife!

No longer, then, thyself disgrace,
In quest of beauty's fame;
No longer, then, expose thy face,
To get thy nose a name.

Take it away, if thou art wise,
And keep it safe at home
Amongst thy curiosities
Of ancient Greece and Rome."

Shakspeare would have thought it high treason; for he says,—

"Down with the nose, take the bridge quite away
Of him, that his particular to forefend
Smells from the general weal."

There may have been many other such noses that have escaped observation,—"born to *blush* unseen:" enough, however, I have here stated of those my recollection furnishes me with at the moment, to establish the fact of variety; and to lead curious physiologists to a scientific classification of this *prominent* and well-deserving feature of the human face. I would recommend a proper distinction being observed between functional varieties, and those which arise from size, shape, or colour, of which, in a cursory way, may be enumerated first—

*Shape**.

Roman.
Grecian.
Snub.
Pug.
Flat.
Sharp.
Bottle nose.
Parrotical nose.

Colour.

Red.
Ruby.
Malmsey.
Claret.
Purple.
Copper.

Now, what does all this come to? *Cui bono?* A great deal for surgery; let us examine what may be done;—we know that noses may be supplied,—may not, therefore, a small one be enlarged, and a large one made small? We have seen a person with a *bunch* of *noses*, but can only, on the authority of Shakspeare, quote one "who had a thousand."

For a great length of time nothing was admired in the world but Roman noses,—and then not a word was heard about them, till William III. brought them again into fashion.

People occasionally possess the power of voluntary action with the muscles of the nose, and can move it horizontally, or to the right and left,—draw it up or protrude it,—so as to make it take any position they please. Painters have been provokingly deceived by this stratagem, and have in vain attempted the portraits of such persons, who were able at every instant to produce a new physiognomy.

One of the qualifications for the Ugly Club, was a nose eminently miscalculated, whether as to length or breadth,—the thickest skin had preference.

Hitherto we have only considered external appearances; we must now notice its functional and other properties.

With some persons, the nose is a sort of barometer,—a certain state of the atmosphere is invariably announced to them by an agreeable sensation of coldness at the tip.

---

* Lavater considers the nose as the fulcrum of the brain; and describes it as a piece of Gothic architecture. "It is in the nose that the arch of the forehead properly rests, the weight of which, but for this, would mercilessly crush the cheeks, and the mouth." He enters into the philosophy of noses with diverting enthusiasm, and finally concludes, "Non cuique datum est habere nasum;"—it is not every one's good fortune to have a nose! A sharp nose has been considered the visible mark of a shrew.

Zimmerman used to draw conclusions, as to a man's temperament, from his *nose!* Not indeed from its size or form, but from the peculiar sensibility of the organ.

Cardan considered acuteness of smell as a proof of penetrating genius, and a lively imagination.

Haller could distinguish perspiration at ten yards distance.

There have been instances on record of blind people who were able to discover colours by the touch, and deaf and dumb, who could feel sounds by placing their hand upon the speaker's mouth: this, however, is not more astonishing, than that the sense of smelling should be so acute, as to enable some persons to judge by it the quality of metals. Martial mentions a person, named Mamurra, who consulted only his nose, to ascertain whether the copper that was brought him were true Corinthian. There have been Indian merchants who, if a piece of money were given them, by applying their nose to it, defined its quality to a nicety, without touchstone, balance, or aqua-fortis. Europeans, also, are to be found whose sense of smelling is equally delicate and perfect.

Marco-Marci speaks of a monk at Prague, who, when any thing was brought him, distinguished, by its smell, with as much certainty as the best nosed dog, to whom it belonged, or by whom it had been handled. It was also said of him, that he could accurately distinguish, in this manner, the virtuous from the vicious. He was much devoted to the study of natural philosophy; and, among other things, had undertaken to oblige the world with precepts on the sense of smelling, like those we have on optics and acoustics, by distributing into certain classes a great number of smells, to all of which he had given names: but an untimely death cut him off in the midst of these curious researches.

The guides who accompany travellers on the route from Smyrna or Aleppo, to Babylon, have no other signs in the midst of the deserts, to discover their distance from the place of destination, than the smell of the sand alone, by which they determine with certainty. Perhaps they judge by the odour exhaled from small plants, or roots, intermixed with the sand.

Physicians, in visiting the sick, have been known to form a prognostic, before having seen the patient, from the effluvia of the sick-room. Those who are in the habit of visiting the insane, know the peculiar odour that characterises that dire calamity; and it was remarked of the plague, that it had " a scent of the flavour of mellow apples."

It is said that monkeys possess this power of discrimination in a very eminent degree. A story is told of a lady who had a pet of this description, whom she made her constant companion, and who suddenly, without any apparent cause, forsook her, and could not be persuaded to re-enter her chamber. The lady was at that time infected with measles, which shortly after appeared upon her; but, on her perfect recovery, the monkey returned to her with his usual familiarity. Some time after, the same lady caught cold, and was apparently very ill, but without fever. The monkey, as far as might be judged from his appearance, seemed to condole with his sick mistress, and to understand the difference of her distempers, by the confidence with which he remained in attendance upon her.

It has even been said, that the sagacity of some dogs has led them to prognosticate the fatal termination of disease. " Whilst I lived at Ripon," says a learned doctor, " I took notice of a little dog, of a chestnut colour, that very often boded the death of sick persons, without being once, for aught I could learn, mistaken. Every time he barked in the night under the windows of any one whose sickness did not even appear dangerous, it happened, infallibly, that the sick person died that week. I knew also," observes the same author, " a man bit by a mad dog, who could distinguish his friends at a considerable distance by the smell, before even he could distinguish them by sight."

So early as the second century, the supplying the deficiency of a lost nose became an object of professional consideration; and the Greeks gave the name Κολοϐώματα, to those who required such an operation. Taliacotius was the first who treated it scientifically; and, from his time, the art of Addition became one of the branches of surgery; and, under the title " *De Decoratione*," formed a very interesting chapter.

Although Taliacotius has the credit of bringing the art of nose-making into fashion, and being the first to write on the mode and manner of performing the operation, yet it appears that one Branca had been in the habit of performing it long before, as we learn from an ancient author, whose name must, in this instance, be considered as the highest authority, being no less a person than Nosorenus.

Why the magistracy of Bologna should have conferred the high honour of a statue on Taliacotius it is difficult to understand,—unless the loss of the nose was of more frequent occurrence than in those days, from the barbarity of warfare and civil punishment; for an old law of the Lombards assigned the loss of the nose as a punishment for theft; and the captives in war were equally spoiled for snuff-takers.

That this was no uncommon dilemma with Italian gentlemen in the fifteenth century, appears by the style in which a Neapolitan poet writes to the *noseless* Orpianus:—"If," says he, "you would have your nose restored, come to me—truly the thing is wonderful. Be assured that, if you come, you may go home again with as much nose as you please."

It does not, however, appear that the nasal operation made any impression on our ancient English surgeons. Wiseman does not even mention it, though slitting the nose, and cutting off the ears, was a common mode of punishing political delinquents in his time; and it is said that Prynne, whose ears were cut off, had new ones made, "*à la* Taliacotius." The fact is, that the operation was misunderstood, and disbelieved, as we know by the jocose manner in which it is alluded to by Butler. It has, however, been successfully revived, and performed, by Mr. Carpue.

Connected with the varieties of the organ of scent, is the well-known story of that extraordinary lusus,

The Pig-faced Lady.

A very few years ago, the town was amused with the account of a pig-faced lady, who lived at Chelsea. The credulous stared, and longed for a peep at her "Ladyship;" while some of the incredulous were not without slight doubts and misgivings, whether an ugly person might not be found, of the nature of a hog. The newspapers, as usual, rang the changes upon this prodigy; and, in the circumstantiality of their details, actually gave a noble pedigree to this pig-gy production. But the fact is, that about once in a quarter of a century, the old story of "MISTRIS TANNAKIN SKINKER" is revived, with embellishments to suit the taste of the times. Now, as it does not fall in the way of every body to read such *old* and such *true* stories as *Mistris Tannakin Skinker*, we will give a short abstract of this "hog-faced gentlewoman," as stated in an original document. Be it known, then, that Miss Tannakin was born at Wirkham, on the Rhine, 1618, and, to the great discomfiture of her worthy papa and mamma, with a face more bespeaking her relationship to the Bacon family, or a cousin of the hogs of Hampshire, than of Christian subjects of a neutral town. The account states, that it was supposed that her mother had been bewitched, and that the daughter could never recover her true shape until she was married.

"If the joy of the parents," says the narrator, "was in the hope of a childe, how much greater may wee conjecture their sorrowes were, to be the parents of such a monster; so they meditated with the midwife, and other women (by which we learn that men-midwives were not then in fashion) that were present at the birth, that they should keepe it secret, as it was possible to doe: and they called the name of it *Tannakin*, which is in English as *Anne*, or *Hannah*."

As might be supposed, she lived, "unknowne in this kind to any one *but her parents and a few other neighbours*." At length her father hearing of a famous artist, an astrologian, named *Vandermast*,—"one suspected to have bin well verst in blacke and hidden arts,"—he very parentally consulted the cunning man, who recommended matrimony as a cure.

To accomplish this desirable end, the worthy parents had much reasoning, *pro* and *con;* and they wisely determined to gild the pill, and therefore they fixed her dowry at forty thousand pounds,—so that the prize, and not the person, should be aimed at.

We can easily believe that this bait made many fish bite, and the doors were beset with suitors. One said, "If she cannot speak, she cannot scold:" another, that her diet would not cost much; therefore, she must be a good bargain.

Amongst the first came a Scotchman, who spent a month's pay on a suit of clothes; and he was highly enamoured of her person; but when he beheld her face, he would stay no conference, but ran away without further answer, saying, "they must pardon him, for hee could *indure no porke!*"

Many others appeared; but all ran away. A souceman of Rumford, on taking leave, said, "I never saw such a hog-snout; but whenever my stomack shall serve for any such dish, I will never venter on any raw; but I will be sure it shall be either well boyled, or rosted." A tailor also declared that he would never stir a stitch in such a business. In short, as the learned author remarks, it would occupy voluminous leaves of paper to shew the various men, of sundry conditions, who came to carry off this "masse or magazine of money."

In order to show how probable this story is, another, very much after the style of "*Beauty and the Beast*," is narrated; and then the whole is concluded with what must satisfy the greater portion of mankind, viz., the *author's own assurance*, that "whoever shall, in pamphlet or ballad, write or sing otherwise than is discoursed of in this small tract, they erre from truth; for what is here discovered, is according to the best, and most approved intelligence."

All these periodical prodigies, even Mistris Tannakin, have their origin in a story current in Paris, 1595; and Brunckman gives another edition of it, in the true history of a Madame Laurussel. To this hour, there is scarcely a provincial fair in the kingdom where some of the Tannakin family may not be seen at the "small charge of one penny!"

Topography of the River Niagara.

[Communicated by Dr. Bigsby.]

The river Niagara fully merits its fame. It is magnificent in dimension, beautiful in form, enriched with various and exuberant foliage, and cheered with bright skies. Its east bank, the north frontier of the United States, is still, for the most part, covered with dense forests; while the Canadian shore presents a succession of hamlets and farms, interspersed here and there with very ornamental gentlemen's seats. As a whole, its great cataract is unequalled. It is annually visited by several thousand strangers of all nations; for whose accommodation three large hotels have been built in the immediate neighbourhood, with every accessary facility that the most fastidious delicacy, or a feeble state of health, might demand for the full enjoyment of the stupendous spectacle.

The superiority of this Fall, however, resides, not so much in its height, and in the grandeur of its surrounding scenery, as in the vast space it fills, and the immense volume of water it discharges. In a picture, it is tame and formal; but in nature these qualities are lost in the general effect, and it becomes permanently attractive beyond expression.

The rapid transition from the placid lake-like character of the river above, to the vehemence and reverberating roar of the Falls, makes also a remarkable impression on the spectator, approaching from Lake Erie*. The great chasm, (at a part of which the superfluous waters of the great lakes pass through a channel only 115 yards broad,) and its picturesque outlet or gorge, are additional features of peculiar charms.

For highly-wrought descriptions of the most interesting scenes on the Niagara, and their effects on the imagination, I must refer to Weld, Hall, Howison, Heriot, and others; the first-named writer being by far the most correct and clear, and, I may add, the most rational. I shall confine myself to plain remarks on the general topography of the river, illustrative of its connexions, size, form, course, and geological structure.

The river Niagara issues from the north-east end of Lake Erie, at the lowest point of its barrier, and enters Lake Ontario on the south-west side, forty-six miles from its head, after having crossed with a general north-by-west course the intervening isthmus, at that point twenty-six and a half miles broad, direct. This isthmus is a portion of the Canadian peninsula, slightly described in the Chapter on Lake Erie in our last No. It stretches easterly from hence, with enlarged boundaries, to the hills of the north limits of Pennsylvania, and of the north and east parts of New York. At present I am only concerned with the neighbourhood of the river Niagara. It is here divided into two levels; the upper advancing from Erie to within seven miles of Ontario. At that distance, it lowers at once in a steep slope, 370½ feet high at Queenston, skirting, at various heights, the whole south shore of Ontario, under the name of the "Parallel Ridge," and, without any very striking change of size in the interval below, bordering the lake.

---

* For effect, this is the best approach.

During the first twenty miles of the upper level from Lake Erie, on both sides of the Niagara, the land is moist, and so flat, as scarcely to assign a direction to its streams. It is elevated but little above Lake Erie, and might be inundated in the spring, were the vernal rise of water as great as in the rivers Ottawa and St. Lawrence. On the Canadian side of the Niagara, and probably on the American also, there is close to it a border of raised ground, varying in breadth from half a mile to two miles, or more.

At the above assumed distance of twenty miles, a very gentle swell commences between Chippewa village and the Falls, and continues waving and in mounds to the brow of the great Queenston slope. It extends westward to the head of Lake Ontario, and is most conspicuous in the group called the "Short Hills," twelve miles from Queenston. Easterly, likewise, at the back of the "parallel slope," this gradual swell is equally perceptible.

The lower level of this interval, comparatively of narrow breadth, is fertile and undulating, watered by numerous rivulets, which descend from above, through woody ravines.

For the purposes of description, I shall divide the river Niagara into two parts, as being above or below the Falls, which are twenty-one miles from Lake Erie. I shall commence with the first of these.

At the head of this river, both shores of Lake Erie are shallow, and have sandy beaches: the Canadian side is fringed extensively with rocky reefs; the American sends out a long spit of sand at the mouth of Buffalo Creek.

All this division of the river is contained by banks of brown loam and clay, mixed now and then with angular fragments of black limestone. At its head they are rather high, and are separated from it by slips of marshy or sandy ground; but they gradually subside nearly to the level of the stream in its course to Chippewa, (eighteen and a half miles,) when a rapid ascent commences.

The direction of the Niagara for three miles from Lake Erie is north, and then bends round to the north-west for two miles, when it is divided into two narrow and distant channels, to within three and three-quarter miles from the Falls, by a very large island. From the foot of this island to the Falls, the river runs west-by-north.

The current for the first three or four miles from the head is swift, especially from near Bird Island to below Black Rock, where it is seven miles per hour. It is smooth in that space, but is much agitated internally. From thence to near the Falls the rate is uniform and moderate. The decline in level from the head of the river to Chippewa is said to be fifteen feet, but upon what authority I know not.

I have few data for the depth of this division of the river: Volney states it generally to be fifteen feet. It is, however, by no means great; and particularly at the lower end, where the shores and islands are often marshy. In the rapids of Black Rock it cannot exceed thirty feet. General Porter* is constructing a basin for shipping of great length there, and occupying a quarter of the breadth of the river: its containing walls rest upon horizontal rock.

The breadth of the river varies much, as is seen from the following statements. Its fluctuations, intermediate to those given below, may be best observed on the map. About the Falls, I may remark, it is a wide expanse, more like a lake than a river.

---

* American Commissioner under 6. 7 a. treaty of Ghent.

	Miles.
Head of the Niagara at Bird Island . .	1.125
Opposite General Porter's house at Black Rock	.375
At the middle of Squaw Island, below Black Rock	.75
At Strawberry Island, nearly a mile below Squaw	1.25
At the head of Grand Island . . .	1.5
At Tonnewanta Island . . .	7.122
At the lower end of Grand Island . .	2.758
At the lower end of Navy Island . .	1.263
At the Chippewa, or Welland River . .	1.125
At the head of Goat Island . . .	.75

This division of the river only contains islands. They are twenty-eight in number; for the most part low and swampy, and finely wooded with sugar-maple, elm, oak, and linden trees, when raised a few feet above water-mark. Their length is usually parallel to the river.

Bird Island, at the head of the Niagara, is a mere ledge of chertzy limestone, opposite Fort Erie, and 400 yards from the east shore: it is from 200 to 250 yards long; and is occasionally used as a shelter from the lake storms.

Squaw Island, a mile and three-quarters below, is close to the American shore, 1880 yards long, by 540 yards in greatest breadth; it is low.

Strawberry Island, 1500 yards lower down, and near the American shore, is a mile and a quarter long, by a quarter in average breadth.

Frog Island, a swamp 300 yards long, is midway between the east bank and the head of Grand Island.

Grand Island is five miles from Lake Erie. As before stated, it creates a great bifurcation of the river. It has always been stated to be twelve miles long, and erroneously: the mistake has arisen from making the estimate along the circumference of the island, close to which (but on the main) the high road passes. On the American shore, this distance is, in fact, thirteen miles and three-quarters, and on the Canadian twelve miles and a quarter. It is seven miles and a half long, and six miles and a half in greatest breadth. Its form is an irregular oval, narrowing greatly upwards. It is only partially cultivated. Its banks rise to the height of from five to twenty feet, and gradually sink towards the interior; much of which is occupied by morass, which in spring forms a series of ponds. It is heavily timbered, where dry. It contains 17,924 acres, according to the survey of the Boundary Commission.

Grand Island occupies nearly all the interval between the banks of the river, the channels on either side being small, and varying but little. That on the east is only 513 yards broad, three-quarters of a mile above Tonnewanta Island; and 660 yards in the breadth of the west channel, three-quarters of a mile below Beaver Island. These are the narrowest places.

I have now to notice in succession,—Beaver Island, on the west side, and three-quarters of a mile from the head of Grand Island—it is oblong, and rather less than half a mile in length; it is 350 yards from Grand Island: the stripe of marsh, 2000 yards long, called Rattlesnake Island, close to the east shore, near the head of Grand Island: Tonnewanta Island, east of, and opposite the middle of Grand Island—it is close to the main, and is rather more than half a mile long. The next is Cayuga, near to the American bank, and to the lower end of Grand Island; it is oval, narrow, and 2060 yards long. Buckhorn Island follows; it is on the east of, and very near Grand Island. It is marshy; 2000 yards long, 400 yards broad at the head, and tapering to a point below. Navy Island is 600 yards from the Canadian shore. It is semicircular; 2000 yards long, and half a mile in greatest breadth: it contains 304 acres. Except a few small ones close to the Canadian shore, it is the only island in this river adjudged by the Commission to Great Britain. Grass Islands, and a few patches of marsh, need not be mentioned here. In a round islet, on the Canadian side of the river, somewhat more than a mile below Chippewa, is a much indented, woody islet, a quarter of a mile in diameter. There is a flat, gravelly islet, surrounded by shallows, 440 yards above the centre of the Horse-shoe Fall; and 300 yards above the Table Rock, on the west side, is a curved, low island of pines, 375 yards long, close in shore. Goat, or Iris Island, is triangular in shape; its base of 400 yards in length, being on the same line with the Cataract. It is half a mile long; and is flat, fertile, and covered with fine clumps of trees: its soil is fine light brown clay, beneath gravel of rolled limestones, and primitive pebbles. About its middle, between it and the American main, there is a round islet, which serves as a point of support for two bridges, connecting Iris Island with the main. On the same side of the river, nearer the Falls, there are seven other islets, growing pines; and near the top, and on the west side, are two others.

No streams of consequence enter the Niagara below the Falls; those above are few, and are sluggish creeks, the discharges of extensive swamps. Of these, the principal are, French, Black, Chippewa, and Tonnewanta Creeks. I shall only notice the two last. Chippewa Creek (or the Welland) pours a dark, discoloured water into the Niagara, nineteen miles from Lake Erie. It rises near the head of Lake Ontario, and passes through a plain, forty or fifty miles long, containing a number of morasses. In this distance, its trifling current varies in its direction with the state of the Niagara. If the winds have caused an elevation in the waters of Lake Erie, (and of the Niagara, by consequence,) the current ascends the Chippewa, and *vice versâ*.

The Tonnewanta is much the greatest stream on the American shore. It rises on the south side of Genesee county, in the state of New York, and is sixty-five miles long, extensive marshes skirting the lower twenty miles. It enters the Niagara near the middle of Grand Island, together with Ellicott's Creek, which comes from the country south of the lower end of Lake Erie.

At Chippewa, we enter from the south the portion of the river more particularly appertaining to the Falls—distant from them two miles and a quarter. A change here commences in the features of the stream. Its ample breadth is sensibly diminished. On the British shore, a ripple in the accelerated current is perceived; and at Bridgewater, one mile lower down, it dashes and foams over a succession of ledges, which are most conspicuous opposite the head of Iris Island. Below this, the water moves with equal swiftness, but smoothly, over pebbly shallows, until it precipitates itself into the great chasm of the Cataract. On the American shore, the rapids begin at a point nearly corresponding with those on the Canadian side. They are very strong at the head of Iris Island; and from thence to the brink of the Falls, leap from ledge to ledge with great fury.

Together with these changes in the condition of the river, the banks, from Chippewa to Bridgewater, very gradually attain the height of forty feet, in inclined scarps of loam, poorly clad with herbage. This apparent rise is caused by the sinking of the bed of the river, to the amount of fifty-eight feet, from Chippewa to the Falls*; but from hence, for the remaining mile and a quarter, a real elevation of the banks and adjacent country takes place. The united effect of these two circumstances is the formation, on the Canadian shore, of lofty steeps, overlooking the Cataract: they are from 120 to 150 feet high; and skirt the river, from Colonel Clarke's, at Bridgewater, in a line of grassy knolls, with here and there an earthy hollow, and highly ornamented with fine trees, among which are some well-grown tulip-trees. Near their commencement, they are separated from the water by a low meadow, a few hundred yards broad; but at the head of the narrow island, in shore, and near the Falls, they rejoin it for a short distance; and finally are lost in the increased elevation of the neighbouring land at the edge of the chasm, a mile below. The American banks have no steeps; but ascend along the river side in a richly-wooded swell.

In this interesting locality, 300 yards below the islet just adverted to, and at the foot of what we must consider as the remains of Iris Island, the Niagara plunges at once into a rocky chasm, 156½ feet deep, 960 yards broad at this place, and prolonged east-north-east, almost at right angles with the former course of the river, for seven miles. This descent takes place obliquely to the direction of the river, and is divided into three distinct falls by Iris Island, and the islet on its right. These are named the "Horse-shoe Fall," "Ribbon, or Montmorenci," and the "American, or Schlosser" Falls, respectively. The whole line of subsidence is 1200 yards long; but the chord of this, joining its extremities, is 960 yards long. The Horse-shoe Fall occupies about one half of the brink, and the base of Iris Island, and the American Fall, each about a quarter. The Ribbon Fall, and an islet adjoining, take up each ten yards of the same line.

---

* Philadelphia Museum, vol. viii. p. 215.

The Horse-shoe Fall is contained by the Canadian shore and Iris Island. Its name is no longer applicable to its form, a correct idea of which, indeed, is only to be acquired with much trouble, from the perspective deceptions arising from the distance of some of its parts. I refer to the sketch for the supposed accurate shape, as laid down in the charts of the Boundary Commission.

A naked, flat ledge, called the Table Rock, at the brink of this Fall, and at its northern end, permits the visiter to dip his feet securely in the water, just as it passes over the precipice. It has been determined, by the officers of the above Commission to be 156½ feet above the surface of the water below. At this Fall, the waters, already in the swiftest motion, are projected in an unbroken curtain*, of an emerald colour in the centre, and white or brown at the sides. They rebound mid-height in large revolving spheres of foam; a small rain spreads around for some hundred yards, and, with the high ascending wreaths of mist, is the seat, in certain circumstances, of brilliant rainbows. The stream beneath the pitch is smooth, but white with internal motion: a little way down, it forms into billows, and maintains great velocity through the whole chasm to Queenston.

At the foot of this Fall, a hundred yards or more from its north end, I am pretty confident that there are very large fragments of rock, which shew themselves a little above water, when the gusts of a tempestuous day have driven away the mist and broken water which at other times conceal them.

The Montmorenci or Ribbon Fall is aptly named. It springs from its dark channel with force, and contrasts very strikingly, by its slender dimensions, with the vast bodies of water around it.

The American Fall is 162† feet high. Its line of descent, although on the whole pretty straight, is in several places slightly serrated. Although the greater part of the water passes down in a continuous stream, yet it frequently dashes upon successive narrow ledges, and then, arching gracefully, drops in broken snowy masses.

---

* The continuation of Table Rock, over which the water passes, juts out beyond the inferior strata, while the curtain of waters also advances beyond the perpendicular line; a space of from fifteen to twenty feet is therefore left between the two, which is largest at the entrance, and is blocked up twenty or thirty feet within by a buttress of rock. The floor of this interval is a slope of slimy fragments of limestone, among which eels are often seen gliding. Violent gusts of wind, torrents of small rain, the slippery footing, and the astounding roar of the cataract, render it a very untenable position.

† Phil. Mus., ut antea.

Stairs have been constructed a little below the Falls on both sides of the river, to facilitate the descent of visiters down the sides of the chasm.

On the British bank, some distance below the Table Rock, a full and almost equidistant view of all the divisions of the cataract is afforded.

Near this spot, at the foot of the contiguous stairs, the scene has peculiar force. Every other object is here shut out. In front, the eye is filled with the expanded curtain of falling waters, the blanched purity of whose colour is rendered more marked by the gloom of the surrounding walls of dark limestone, crested with hemlock, pines, and elms. Colossal fragments in magnificent confusion mount half-way up the precipices, and even obstruct the stream, rolling impetuously.

A slippery path over these fragments on the north side leads to the foot of the Horse-shoe Fall, whose depth of curvature and very great length is there more fully developed.

We now proceed to bestow a few words on the lower division of this river.

The chasm is six miles and three-quarters long, and for two miles from the Falls runs east-north-east, when it turns to the north-west; a mile farther, to the whirlpool. It there changes suddenly to the north-east, and so continues, or with slight variation, for two miles, to a little beyond the Devil's Hole, on the American side; from thence a northern course is slowly assumed, and, with a few jutting elbows, is continued to Lake Ontario. The whirlpool above alluded to is a circular basin, 500 yards in diameter, on the left side of the river, created by the sudden change in the direction of the river. Its violence has been exaggerated. The water rushes into it in billows from the pent channel above, and then, with eddies here and there, courses round the Canadian side of the basin in a swift smooth current, and so flows off.

The side of the chasm may be described in general terms to be precipices, often, as near the Falls and elsewhere, mural in the upper half of their height or more, and terminated below by slopes of fragments, naked or overgrown with vines and other creeping plants; but more commonly they consist of ledges, and enormous displaced masses, numerous or few, high or low, according to accidental circumstances, and being interspersed with patches of soil, are clothed with underwood and fine trees. When the sides of the cliff are naked, the bright and contrasting colours of their rocks have a very shewy appearance. When they are mural, these colours are in bands.

On the Canadian side, a mile and a half below the Falls, there is a cave called the Devil's Hole. Some roots adhering to the rocks afford the means of descending to a ledge twelve feet below the summit of the precipice. This ledge is the floor of the cave, which is twenty feet square, six feet high, and has a large mouth. The roof is uneven, and is covered with damp mould. There is another cave on the American side, two miles above Lewiston; I have not seen it.

The average breadth of the river in the chasm is 300 yards; but a mile below the whirlpool it contracts to 115 yards, near the ruins of a saw-mill. Here the bottom is seen, nearly in mid-channel, to consist of large fragments of rock, over which the water passes with inconceivable fury. The precipice on the Canadian side is so shattered here, that with some ingenuity and labour, an indifferent cart-road has been made down it. Two miles and a half below the whirlpool, the breadth of the floor is 135 yards. Rather more than half a mile above the gorge of Queenston, it is 130 yards broad, and at the gorge 212 yards.

Of the depth of water in the chasm I know very little. Mr. Forsyth, the proprietor of the two hotels at the Falls on the British bank, informed me that, in the middle of the basin, in front of the Falls, the depth is 160 feet.

The bed of the river makes a gradual descent of sixty-seven feet* from the foot of the Falls to Queenston gorge. At this last place, therefore, the sides of the chasm are higher than at the former. Mr. Gourlay states the elevation of Queenston heights to be 370½ feet, and, I believe, correctly.

---

* Philad. Mus., ut antea.

The scenery in the west side of this great defile is extremely rural and pleasing; but the view from the monument to General Brock, on the brow of the heights just noticed, is uncommon, as well as extensive and beautiful. In the immediate foreground, 370 feet below, are two pretty villages—Queenston and Lewiston; between them the Niagara, escaping from the chasm, expands into a tranquil river, and is traced in gentle winding through a mass of woods, seven miles across, till it loses itself at Fort George in Lake Ontario, whose wide waters, bounded on the north by blue heights, are seen in the distance. The junction of the river with the lake is marked by the heavy white building on the east bank of the former, called Fort Niagara, and the town of Niagara on the west bank, containing a thousand souls, and built in rectangular streets on a pleasant plain.

The suddenness of the depression at Queenston of the upper or Erie platform causes the escape of the Niagara from the chasm to be as sudden; but the widening of the stream, now succeeding, is slow, and from opposite Lewiston to Lake Ontario it seldom varies from a breadth of 700 yards. The course of the river is rather west of north: its current strong—averaging perhaps two miles per hour. The banks are of schistose clay and argillaceous sandstone at Queenston and Lewiston, supporting a gravelly loam, and are from fifty to eighty feet high; but from thence to the mouth of the river they are a rich red clayey soil, and preserve a considerable elevation throughout. At the contiguous shore of Lake Ontario, they are from twelve to fifteen feet high, of pure clay below, covered with primitive boulders and a mixed soil. The confluence takes place with little preparatory expansion. The width between Fort Niagara, and the town of that name, is from 800 to 1000 yards. There is a considerable bar of sand and mud off the mouth.

Geology of the River Niagara.

The diluvium and alluvium around the River Niagara afford but little room for remark, and have been sufficiently noticed in the geographical portion of this paper, and while treating on Lake Ontario. There are in this vicinity few boulders, and those of small size. Instead also of belonging to the districts in the west and south, they are all (as far as can be ascertained) from the east and north. The majority are primitive, but of characters so common that it seems impossible to assign their home. The red sandstone of the lower regions on the east is usual on the upper level of this river, together with the milky quartz rock of the outlet of Lake Ontario, and the arenaceous limestone from the same quarter; the latter characterised by micaceous scales, and its peculiar fossils—the plagiostomæ, &c.

It is a remarkable fact, and supplies another proof (if it were wanted) of the great chasm being the work of the present river, that there is scarcely a travelled rock in it. I met with one only—a large fragment of gneis, full of small garnets, quite that of the Portage "*Chaudiere des Francois*" on the Lake Nipising. It may have been brought from the upper lakes in the blocks of ice which every spring are precipitated over the Falls.

The chertzy limestone at Black Rock, already described, extending into Lake Erie, two patches of the brown limestone beneath it, a little south of the village of Waterloo, on the British shore, and a small interval of argillaceous shale, below the Gorge of Queenston, are the only fixed rocks on the River Niagara, out of the chasm.

In that deep and often mural defile its stratification is beautifully developed, although seldom accessible. An examination of the face of the "Parallel Ridge" (through which it is cut) along its course to the head of Lake Ontario, shews that similar rocks underlay the districts in the west for many miles; and we shall find them to continue easterly to the primitive ranges at the east end of Ontario.

The walls of the chasm are composed (beginning from above) of the brown limestone over which the Falls pass. This is incumbent on calcareous shale, which, again, rests upon a series of arenaceous rocks, highly ferruginous in the superior portions, and in the inferior very argillaceous, and contain springs of brine. Of this last kind is the red shale, supporting the village of Queenston, and not exceeding two feet above the level of Lake Ontario.

The brown limestone, just mentioned, supports the chertzy variety at Black Rock. Under the same relations that it observes here it crowns the parallel ridge throughout the south-western half of Lake Ontario to its head, and of course underlays the country on its south. It is from twenty-five to twenty-seven yards thick, near the Falls, including some ledges contiguous to, and behind the precipice.

It is commonly fetid, of a dark brown colour, but sometimes light grey. Its texture is fine and coarse grained, and in certain layers is full of minute ragged cavities. The layers are often exceedingly thick*. Beneath the Table Rock, the lowest is thirty feet thick, or thereabouts; but subdivides suddenly close at hand. At the Upper Staircase, on the British side, the top stratum is sixteen feet in thickness; but in like manner it breaks up in both directions into a number of layers from one to three feet thick. They are very massive and light coloured near the summit of the "Parallel Ridge," one mile and a half west of Queenston. The stratum, noted as being nearly thirty feet thick, is traversed horizontally by at least six singular fissures or rifts, whose upper and under surfaces interlock firmly by rugged masses and points, overlying each other exactly as in the sutures of the cranium. The parts in contact are extremely smooth, bright, and black or brown. The prominences, sometimes one and two inches long, are usually fluted or grooved longitudinally. In hand specimens, with a little violence, this kind of dove-tailed joint can be taken asunder, and replaced. I did not observe it in the other layers around the Falls, but it occurs repeatedly down the chasm, and is very distinct in the grey limestone on the way to St. David's.

Some curious appearances present themselves at the junction of this limestone with the black shale below,—the latter there becoming rapidly thicker and browner. The bottom of the limestone, for a foot or so, is occupied by flat masses of black limestone, (apparently of the layers below,) oblong, almost all lying horizontally, with their edges rubbed off, but never more than six inches in length. They are numerous, but usually separate. For several feet upwards, minute fragments (mere dots) of this limestone pervade the stratum.

---

* It contains two small caverns: one is on the British side, a mile and a half below the Falls.

I have seen this appearance only under the Table Rock. This brown limestone has been named "geodiferous" by Mr. Eaton, from its abounding remarkably in drusy cavities and geodes, lined with various crystallizations. At the Falls, they are rare in the lower layers; very numerous in the upper third; and, near the top, often constitute the greater part of the rock. These crystallizations are of pink pearl spar—nearly cubic rhombs; trihedral and hexedral pyramids of calcspar, (hyaline,) foliated selenite, purple fluor, crystals of quartz, and slender prisms of the sulphate of strontian. These minerals are either solitary, or grouped together in the same geode. Considerable masses of yellow blende are common, imbedded (with a quartzose covering) in the limestone, singly and with galena: occasionally, the most ordinary form of the crystal of the former is met with in the geodes. Large and small lumps of granular gypsum are frequent in the lower layers, but not so above—their place seeming to be taken by a vast number of siliceous nodules, from two to seven inches in diameter. These are often nearly round, with a dark rough surface; and are formed of concentric layers of calcareous flint, which are either compacted into a greyish-veined substance, or are, in parts, distinct; their surfaces being lined with crystals of quartz, selenite, and calcspar; the latter occasionally of a bright salmon colour, or a delicate yellow. I believe them all to be the work of madrepores, as their cells are often very evident here; and still more so at Rochester, on the Genesee river. These nodules, the geodes and their minerals, are almost altogether wanting in the vicinity of Queenston, and in the whole course of the "parallel ridge," to Grimsby at least; but they are plentiful on the south side of Ontario.

Organic remains in this limestone are very rare at the Falls. At the Table Rock, I met with a few terebratulæ, many hollow casts of the fragments of encrinital columns, and one turbo. Lieutenant Bolton found another turbo. There are some nests of favasite. As well as the very unfavourable circumstances of position, &c., would allow me to observe, little change in this rock takes place on the Canadian side, to within about a mile and a half of Queenston; from which distance to the gorge of the chasm it occupies a retreating cliff, almost, if not altogether, unapproachable. The inequalities, however, of the brow of the mountain, about Brock's Monument*, and along the summit of the cliff, relieve us from these difficulties. The broken platforms and short ledges of this brown limestone which here prevail, shew it to be at least seventy feet thick. It is full of encrinital fragments—many of them coloured of a fine red, like those of Lockport, on the south shore of Lake Ontario. The large bicarinated terebratulæ of Lake Erie are not uncommon; and the debris of indeterminable trilobites, similar to that of Lakes Huron, Sincoe, and Ontario.

I saw many turbinoliæ, ramose millepores, retepores, cellular madrepores, one turbo, and well-marked productæ. The suture-like fissures noticed at the Falls are met with; and, I think, in the same stratum. I did not see here one nodule of gypsum or of silex: but the limestone of the Ridge, one mile and a half west, at the same height, has numerous irregular cavities, lined with crystals of quartz and calcspar; and imbedded in it are the hollow encrinital moulds before spoken of, and a few turbinoliæ. In the lower layers here, too, are the fragments of trilobites, with terebratulæ, corallines, &c.

Examining the Ridge at Wolverton's, thirty miles hence, and a mile or more west of the village of Grimsby, I found no change whatever in this limestone; but I saw none of the nodules above mentioned, probably from the layers in which they chiefly prevail being concealed by herbage. The rock on which this brown limestone is placed, is a black argillo-calcareous shale. It is visible throughout the chasm; along the face of the Ridge to Grimsby, on the west, it can scarcely be seen, for the herbage covering it, nor did I meet with it. I think it positively wanting at Wolverton's. The continuation of those heights easterly, display it in many places; among which Lockport and the Genesee Falls are the best known.

It is very shaly at the Falls of Niagara, but has browner layers, six to twelve inches thick, interleaved at regular distances. It is homogeneous, and effervesces on exposure to acids. It is often spotted yellow by sulphur, which, under the arch of the Falls, may be gathered in tufts of very small acicular crystals. On this river, and in its vicinity, it does not contain a trace of animal or vegetable life; but at the two places above spoken of, it abounds with new and important kinds of the former, as well as an abundance of the more common. The thickness of this stratum has not been ascertained on the river Niagara; nor can it be, I think, without much trouble and expense. Its inferior connections have not been examined at this place, further than to determine, that the next rock succeeding below is a series of sandstones, which I shall immediately proceed to describe; leaving the additional facts respecting the calcareous shale to be brought forward when on the geology of Lake Ontario.

---

* This fine column is near the highest part of Queenston Heights.

These sandstones are found throughout the country, west and south of Lake Ontario, little changed in mineral characters or geological position. I do not think that their contents, if thoroughly examined, would present any great differences; but at present, certain organic remains seem to be confined to particular localities. They are every where distributed into the following subdivisions. The first species (from above) is a grey, or white quartzy sandstone ("ferriferous sand rock" of Eaton). It is followed successively by a red argillaceous slaty sandstone, ("ferriferous slate" of Eaton;) a hard, grey calcareo-argillaceous sandstone, ("grey band" of Eaton;) and a soft red and green argillaceous shale, to which Mr. Eaton restricts the property of being saliferous, and names it "saliferous rock."

I am inclined to consider all these strata, and their superincumbent limestones, to have been deposited nearly at the same time. They are all conformable to each other, and rarely betray marks of disturbance at the connecting planes. The contained fossils indicate no change of era—the two upper sandstones sometimes alternate. All these sandstones are finely displayed at one place or other in the chasm; and best on the American side, in the middle and lower parts. Near the Falls, the debris of the surrounding cliffs creeps up nearly to the top of the black shale, and only permits a little of the red sandstone to appear at the water's edge; but about the whirlpool, and especially opposite the Old Mill, the brown, black, grey, and red stripes in the mural precipice point out, in a clear manner, the size and relative position of its component strata. They are also well seen on the east side of Queenston Gorge, where, particularly, the saliferous shale comes into view, and supports the villages of Queenston and Lewiston, in abraded banks, eighty feet high.

The grey sandstone is a grey or greyish white aggregate of uncemented grains of quartz, here and there spotted with iron-rust. It is in very thick strata, and is best seen in the face of the heights close to Queenston, where it is quarried. Mr. Eaton found it to be fourteen feet thick here, and in the river Genesee. It contains no fossils.

This rock is hard, breaking into thick, shapeless, or squared blocks. "In some places, it alternates with the red rock below; but generally they are separated by a continuous layer, or extensive bed of argillaceous iron ore," (Survey, p. 120,) as at Niagara, according to Mr. Eaton. The red argillaceous sandstone beneath this rock is very shaly. It is often soft and almost homogeneous; but frequently, in the contiguous layers, it is harsh and granular. It is about twenty-three feet thick, (Survey, p. 121;) its colour is principally red, variegated in clouds, spots, and circlets, with green and blue colours, which occasionally occupy considerable thicknesses of the shale. It is extremely ferruginous, both as disseminated generally, and in bluish-black knots, with a semi-metallic lustre. It contains terebratulæ, both large and small; and a fossil which resembles somewhat the orthocera annulata of Sowerby; but it varies from it in being much more narrow, in having its sides compressed into a quadrilateral form, and in being traversed by minute and numerous ridges. Casts of shells, apparently unios, in red sandstone, are common. The calcareo-argillaceous sandstone, occurring next, evidently belongs to the saliferous formation, like a similar stratum at Runcorn and Manley, in Cheshire.—*Geol. of England*, Coneyb. and Phill., p. 280. It is here a moderately hard granular rock, chiefly of quartzose materials. It is white, chequered with flakes of green. It is thin, but is continuous on the east for 200 to 250 miles. It is only eight feet thick here. It contains exquisitely fine lingulæ mytiloides, in which the original shell is beautifully preserved. There also are many thick white casts of shells, of the unio family. The variety of orthocera, above noticed, is also found here in white casts. The two latter, and the terebratulæ of the red sandstone, are found near Grimsby.

The red saliferous rock, principally argillaceous, occupies the lower seventy or eighty feet of the Gorge of Queenston, and extends a considerable distance into the expanded river below. Its mineralogical characters are quite the same as at Salina, on the south of Ontario; and as at St. Catharine's and at Saltfleet, near the head of that lake. At all these places, its geological relations are the same. At the two latter, it appears at the foot of the "Parallel Ridge," which is composed of the same materials as in the chasm.

This rock presents no peculiar characters on the river Niagara. It has been most fully studied by Mr. Eaton on the borders of Lake Ontario. I therefore refer the reader to its description in the paper on Lake Ontario, where the Salt-works of St. Catharine's and Saltfleet will be more appropriately introduced than here.

JOHN J. BIGSBY.

On the Relation between the Density, Pressure, and Temperature of Air; and on Experiments regarding the Theory of Clouds, Rain, &c.; with a Conjecture about Thunder and Lightning. By HENRY MEIKLE, Esq.

[Communicated by the Author.]

FEW things, I presume, would tend more to facilitate and promote the study of pneumatical and meteorological science than an acquaintance with the relation between the density, pressure, and temperature of air. Preparatory, therefore, to entering on the consideration of some meteorological phenomena, I submit the following attempt towards investigating that relation.

1. It was inferred from Boyle's experiments, and subse-

quently confirmed by others, that, under the same temperature, the elasticity or pressure of air is as its density, or inversely as its volume; and, consequently, while air undergoes the same change of temperature, its volume varies under a constant pressure, precisely in the same proportion as the pressure would do, were the air confined in an inextensible vessel.

2. Mr. Dalton, M. Gay Lussac, &c. have ascertained, that on heating air, under a constant pressure, from 32° F. to 212° its bulk acquires an increase of three-eighths. Such increase in the air-thermometer is divided into 180 equal parts, or degrees, for Fahrenheit's scale; and the like divisions, corresponding to equal variations of bulk, are continued both above 212° and below 32°. Now, as three-eighths to 180°, so is the whole bulk at the freezing point to 480°; and, therefore, there cannot be more than 480 degrees below 32° on the Fahrenheit scale of an air-thermometer; for by taking 480 such equal parts from the bulk, the whole would be exhausted*.

3. The freezing point being marked 32°, it is obvious that 480° below this will be at $-448°$. The bulk of a given mass of air, therefore, under a constant pressure, varies as its temperature reckoned from $-448°$, according to the common graduation; that is, as $448°+t$. The degree indicated on Fahrenheit's scale being t. Hence, by art. 1, the pressure of air confined in an inextensible vessel likewise varies as $448°+t$.

4. It has been more recently found, that, whilst air undergoes the same alteration of temperature, the change in its quantity of heat is one-third greater under a constant pressure than if confined in an inextensible vessel†; also, when the

---

* Hence, unless we make the incomprehensible supposition, that air may contract by cooling, till its volume be less than nothing, the *absolute* zero, if such a thing exist, cannot be lower than $-448°$ F., with reference to the common graduation of an air thermometer; no matter what relation the degrees bear to true ones. But however clear this may be to most people, Mr. Ivory's new law of condensation and rarefaction, to be shortly noticed, recognises no such limitation; but readily produces a cold of *thousands* of degrees below the *impassable* limit of that very scale which he maintains to be the true one. In short, his law is fraught with contradiction, view it which way we may. This shews the propriety of examining and trying the consistency of everything, even though it should have issued from an oracle.

† Omitting this property, all that is contained in the first six articles would hold, without regard to the nature of heat, provided the experiments, especially those to be after noticed, be correct.

density of air suffers a minute and sudden change, such variation of density is to the whole density as three-fourths of the accompanying variation of pressure to the whole pressure; or, ρ being the density and p the pressure,

$$d\rho : \rho :: \frac{3}{4} dp : p \text{; and } \frac{3dp}{p} = \frac{4d\rho}{\rho}.$$

Consequently, one-fourth of the minute variation of pressure must be due to change of temperature; for (art. 1) when the temperature is constant, the pressure varies exactly as the density; and hence because (art. 3) p varies as $448^\circ+t$ for temperature, we have

$$\frac{dt}{448^\circ+t} = \frac{dp}{4p} = \frac{d\rho}{3\rho}.$$

5. The equation

$$\frac{3dp}{p} = \frac{4d\rho}{\rho}$$

evidently expresses the relation between the fluxions of the logarithms of the pressure and density of air, when its quantity of heat is constant. The fluent is

$$3 \text{ log. } p = 4 \text{ log. } \rho + \mathrm{C};$$

and if p' and ρ' be put for the initial values of p and ρ, we have $\mathrm{C} = 3 \text{ log. } p' - 4 \text{ log. } \rho'$, and

$$3 \text{ log.} \frac{p}{p'} = 4 \text{ log.} \frac{\rho}{\rho'} \text{; or } \left(\frac{p}{p'}\right)^3 = \left(\frac{\rho}{\rho'}\right)^4$$

which is the relation between the pressure and density of air, when its quantity of heat is invariable; viz., that the cube of the pressure varies as the fourth power of the density.

6. Also, by integrating the equation

$$\frac{dt}{448^\circ+t} = \frac{d\rho}{3\rho}$$

and putting t' for the initial temperature, we get

$$\frac{448^\circ+t}{448^\circ+t'} = \left(\frac{\rho}{\rho'}\right)^{\frac{1}{3}} \text{; or, } t-t' = \left(448^\circ+t'\right)\left[\left(\frac{\rho}{\rho'}\right)^{\frac{1}{3}} - 1\right]$$

$$= \left(448^\circ+t'\right)\left[\left(\frac{p}{p'}\right)^{\frac{1}{4}} - 1\right]$$

for the change of temperature on the common scale solely due to change of density*. Neither this conclusion, nor that which terminates article 5, necessarily depend on any theory of heat.

---

* In the seventh Number of the Quarterly Journal of Science, I adduced a few examples to shew what an incoherent rule Mr. Ivory had given for estimating the change of temperature in air due to a change of

7. If the areas which the curves ABC and DEF intercept from the straight line GH, between the same perpendiculars to GH, be everywhere to each other as 4 to 3; and, G being a fixed point, if every three perpendiculars ADH, BEI, CFK, which make $\left(\frac{GI}{GH}\right)^4=\left(\frac{GK}{GH}\right)^3$ likewise make area BIHA= FKHD, the curves are hyperbolas.

For if not, with G as a centre, and GH as an asymptote, describe two rectangular hyperbolas, *a*B*c*, *d*E*f*, cutting AH and CK in *a*, *d*, *c*, *f*. Then all the ordinates of either hyperbola being, as is well known, inversely proportional to their distances from G, the corresponding ordinates of the two hyperbolas are everywhere to each other as BI : EI :: 4 : 3;

---

its density, and how directly it is opposed to observation. This I instanced, both so far as regards the inconsistency of the results when we revert the process, and likewise when we compare the change of temperature computed at one operation with what it would be, if computed by the same rule, supposing the whole change of density to take place by successive steps. At present, I only beg to call the reader's particular notice to the circumstance, that the smaller these successive variations of density are assumed, the nearer will the aggregate results or changes of temperature, obtained by the continued application of Mr. Ivory's theorem, approach to perfect consistency; whether among themselves, or when we retrace our steps by reversing the process. Now such continued summation of minute variations of temperature being similar to integration, shews that Mr. Ivory's error consists in his using the *fluxion* of the time as its *fluent*; or at least in assuming the fluent to be proportional to, or a mere multiple of the fluxion. Thus $t-t'$ being the change of temperature caused by $\varrho-\varrho'$ the change of density, if we take these changes as differentials, Mr. Ivory's theorem, viz.

$$t-t'=\frac{3}{8}\left(448^{o}+t\right)\times\frac{\varrho-\varrho'}{\varrho},\ \text{becomes}$$

$$dt=\frac{3}{8}\left(448^{\circ}+t\right).\frac{d\varrho}{\varrho};\ \text{and}\ \frac{dt}{448^{\circ}+t}=\frac{3}{8}.\frac{d\varrho}{\varrho}$$

which, with the exception of the constant $\frac{3}{8}$ for $\frac{1}{3}$, is just the fluxion we had in art. 6; and therefore the integration gives

$$t-t'=\left(448^{\circ}+t'\right)\left[\left(\frac{\varrho}{\varrho'}\right)^{\frac{3}{8}}-1\right]$$

a self-consistent result, but as different from Mr. Ivory's new law of condensation as light from darkness. The notation in my paper above quoted only differs from this, in that I there used i and unit, in place of $t-t'$ and ϱ' here.

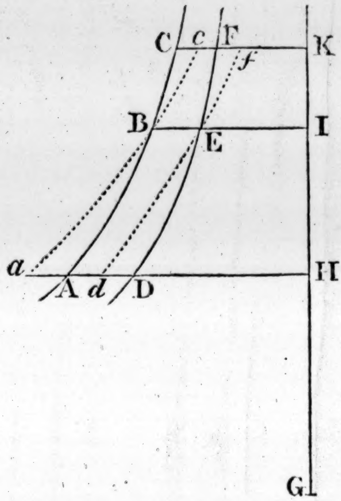
and therefore so are their respective areas between the same parallels. But another known property is, that, in the hyperbola aBc, area $cKHa : BIHa :: \log. GK - \log. GH : \log. GI - \log. GH :: 4 : 3$; as is evident by taking the logarithms of the last equation but one, and stating them in a proportion. Hence, area $BIHa = \frac{3}{4}(cKHa) = fKHd$; proving, conversely, that hyperbolas possess both the properties specified in the proposition. By omitting area $EIHd$, there remains $EIKf = EBad$; and, in the same way, $EIKF = EBAD$.

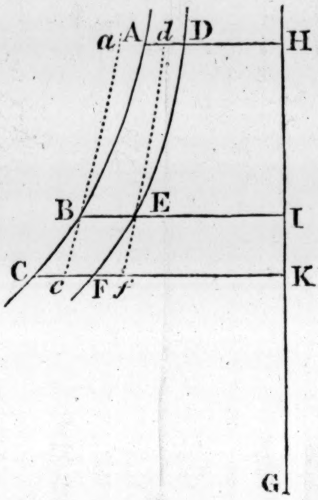
If $Kc < KC$, and $Hd > HD$, and if the hyperbolas do not again meet the other curves between AH and CK, we have area $BIHA < BIHa$. But $EBAD = \frac{1}{4}(BIHA)$, and $EBad = \frac{1}{4}(BIHa)$; wherefore, $EBAD < EBad$, and yet $EIKF > EIKf$; that is, two equals are respectively greater and less than two equals, which is absurd. The like would follow, were $Kc > KC$ and $Hd < HD$.

In however many points the hyperbolas might cut the other curves, yet three ordinates may always be taken so near to each other that the same absurdity will occur as before. For the middle ordinate BI, and either of the others, as AH or CK, may be drawn at pleasure, and the position of the third obtained from

$$GK = \left(\frac{GI^4}{GH}\right)^{\frac{1}{3}}; \text{ or, } GH = \frac{GI^4}{GK^3}.$$

It is, indeed, a supposable case, that the hyperbolas might touch the other curves in B and E; but these curves cannot be touched in every point by hyperbolas having all the same asymptotes. Suppose, however, the curve ABC were touched by two such hyperbolas; then, because the latter do not meet, some third hyperbola lying between them could cut the curve ABC in at least one point, B'; and because ABC and DEF cut the ordinates in the same ratio as the hyperbolas do, the curve DEF would be cut by its corresponding hyperbola in a point, E', situated in the same ordinate with B'. Other two ordinates, therefore, being drawn near enough to $B'E'$, would still produce the former absurdity. Hence, ABC and DEF can only be hyperbolas.





Let A B C (using the former figures) be a curve, such that whilst the temperature of a mass of air, under a constant pressure, undergoes any change indicated by H I on the scale of an air-thermometer, the corresponding change in its quantity of heat may be denoted by area B I H A, intercepted between the ordinates B I and A H perpendicular to H I ; and let D E F be a similar sort of curve, making area E I H D $=\frac{3}{4}$ (B I H A), which, therefore, represents (art. 4) the change which the quantity of heat would have undergone had the air been confined in a close vessel during the change of temperature H I. Let the common scale of the air-thermometer, of which H I is a part, be continued downward to the point G, answering to $-448°$ F. ; then (art. 3) the volume had changed under a constant pressure in the ratio of G H to G I, whilst the temperature shifted from H to I. But now, suppose the air, which has acquired the temperature I, and undergone a change of heat $=$B I H A, to be instantly restored to its original volume, whereby the temperature reaches the point K on the scale. It is evident, that the air is now brought to the same temperature as if its quantity of heat had undergone the change F K H D $=$B I H A, with its original volume all the while invariable.

During the restoration of the air to its original volume its quantity of heat is supposed constant, but the density has been changed in the ratio of G H to G I, and the pressure (art. 1) in the same ratio compounded (art. 3) with the ratio of G I to GK for change of temperature ; that is as G H to G K.

Hence, (art. 5) $\frac{\text{GI}}{\text{GH}}=\frac{\varrho}{\varrho'}$; $\frac{\text{GK}}{\text{GH}}=\frac{p}{p'}$; and, therefore, $\left(\frac{\text{GI}}{\text{GH}}\right)^4=\left(\frac{\text{GK}}{\text{GH}}\right)^3$. But we had also BIHA=FKHD; consequently (art. 7) the curves A B C, D E F are hyperbolas*.

Since the variations of the area denote variations in the quantity of heat, whilst the corresponding variations of the abscissæ denote variations of temperature on the common scale, it follows, from the known property of the hyperbola, that whilst the variations of the quantity of heat in air under a constant pressure are uniform, the variations on the common scale of an air-thermometer form a geometrical progression. Or more generally,—when the variations in the quantity of heat are uniform, those of the volume under a constant pressure form a geometrical progression; as do likewise the variations of pressure under a constant volume†.

Having finished this investigation, it may be useful to give some account of the experiments alluded to in art. 4. I shall first give a familiar illustration of the principle, and then state more nearly the actual mode of experimenting.

---

* These curves would still have come out hyperbolas, if, throughout this investigation we had used, in place of 4 and 3, constant symbols, or any two numbers of which the first exceeds the second: so that the scale of the air-thermometer now investigated is not the consequence of a particular ratio between the areas of the two curves. It only requires a *constant* ratio. Now the *constancy* of this ratio (as will be noticed after) is strenuously contended for by Mr. Ivory and the French mathematicians, who, therefore, set out with the same data as I do. But in place of going through with the investigation of the scale of temperature, for which the above data, if true, are, as we have seen, amply sufficient, these great mathematicians stick fast in the middle of it, and *assume*, what cannot be proved, because directly at variance with the data,—*that the common scale of the air-thermometer is the true one.* Among many other absurdities, of which Mr. Ivory's new law affords a notable example, this *assumption* would require the above curves to be mere straight lines!—See *Mécanique Celeste*, tom. v., p. 128; *Annales de Chimie*, xxiii., 337; *Phil. Mag.*, Nov. 1823, and Feb. 1827.

† I flatter myself that the investigation now given of the scale of the air-thermometer, may be easily followed by such as are but little conversant in fluxions, provided they know a few of the simplest properties of the hyperbola. I have been the more sparing of fluxions, and have employed geometry for another reason; namely, because Mr. Ivory having egregiously mismanaged this problem when using fluxions, is anxious to darken the subject, and to shelter himself in pretending, that the problem is necessarily *indeterminate* or ambiguous in its result. Whoever attends to the above solution will readily see that, if once the data be admitted, there is no alternative as to the result.

Suppose a close vessel to contain air of the same temperature and pressure as that in the apartment, and to be furnished with a gauge for showing any minute change of pressure. In this state, let it be carried into another room 4° warmer, when the gauge will soon indicate an increase of elasticity, which we may call 4°. If the vessel admit of being now opened sufficiently, and shut again so promptly, as just for the moment to allow the included air to regain the external pressure, without affording time for its abstracting heat from the vessel, the gauge will in a little time indicate an increase of 1°.

The reason of this may be traced, in a general way, to the known circumstance that air, of whatever density, is cooled by dilatation. The gauge, while indicating 4°, shews the included air to be denser than the external. On opening the vessel, therefore, the superior elastic force within expels a portion of air, leaving the remainder, of course, rarer, and consequently cooled, as the result shews, one degree below the temperature of the second room. At the moment of re-shutting the vessel the elasticity of the included air is in equilibrio with the external pressure; but it must then be more dense than the air of the second apartment, otherwise, its temperature being lower, it could not balance the external pressure. On regaining, therefore, the lost degree of temperature, the excess of density shows itself by the gauge. The escape of a small portion of air does not alter the result, because it only carries off its own heat.

Hence, the quantity of heat which had kept up the temperature of the air 4° above its original pitch while confined in the vessel, shows itself to be only able, under the original pressure, to maintain an excess of 3° above the original temperature. The quantity of heat, therefore, which would change the temperature of air one degree, and which is usually called its *specific* heat, is one-third greater when the air is free to change its volume under a constant pressure, than if confined in an inextensible vessel*.

---

* By *specific* heat, some profess to mean the absolute or whole quantity

The experiment would have been essentially the same had the vessel been only closed under the temperature and pressure in the second apartment, and then air injected till the gauge stood at 4°, after the temperature had settled. The rest of the experiment, as to opening and shutting, being as before described. Now, in either method, it is obvious that, at the instant of re-shutting the vessel, the included air, with reference to pressure, had lost all the 4°; while, as the second indication of the gauge shews, it had only lost 3° with regard to density,—one of the lost degrees of pressure being due to temporary depression of temperature. Hence, the height of the barometer, or whole pressure, is to the variation of pressure denoted by 4° as p to dp. Also, the height of the barometer, or whole density, is to the variation of density denoted by 3°, as ϱ to $d\varrho$. Consequently,

$$p : 3\,dp :: \varrho : 4\,d\varrho.$$

This is nearly the mode of experimenting which I followed. It had been previously practised, through a great range both of temperature and pressure, by MM. Gay-Lussac and Welter; but they do not make the simultaneous variations of density and pressure exactly as 3° to 4°,—which is very nearly the ratio I obtained from a mean of many experiments,—but as 3 to 4.1244. The difference is not considerable, and there is one consideration which, I think, renders it extremely probable that the former is the true ratio*. Sir Isaac Newton has shewn, (*Principia*, lib. ii., prop. 23,) that if, in an elastic fluid, the cube of the pressure vary as the $n+2$ power of the density, the particles will repel each other with forces inversely proportional to the nth powers of their distances; and the same may be seen in other elementary works. Now, these experiments

---

of heat which a body contains. So long, however, as nothing is known, or likely ever to be known, of the absolute heat in bodies, it is useless to have any term for it, and it can only breed confusion to use a term which has commonly a very different meaning.

* I first used a vessel holding about ten gallons of air, and which can be both opened and shut in a small fraction of a second. The opening amounts to 2·88 square inches. I have since repeated the experiments with a vessel of nearly the same size, but having an opening of 25 square inches. With this I intend making a variety of experiments connected with the subject.

of the French philosophers just named make the cube of the pressure proportional to the 4.1244 power of the density, (as is evident by using 4.1244 in place of 4 in art. 5). Hence, $n=2.1244$; and, therefore, the particles should repel each other with forces inversely as the 2.1244 powers of their distances,—a law of which there is no known parallel in nature *. But if the ratio were that of 3 to 4, and consequently, as in art. 5, the cube of the pressure varying as the fourth power of the density, the repulsion between the particles would be inversely as the *squares* of their distances, which is the only known law of repulsion, and most likely the only one which exists.

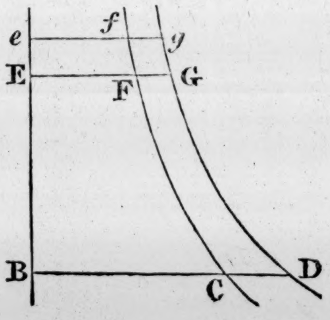
It appears from experiment, and has been laid down as an ascertained principle, by Marquis Laplace, M. Poisson, and M. Ivory, that the specific heat of air, or the quantity of heat which raises its temperature one degree, under a constant pressure, bears an *invariable* ratio to its specific heat, when confined in an inextensible vessel. None of these philosophers seem to have been at all aware of the necessary and unavoidable consequences of laying down this principle: one of which, as we have already seen, is, that it requires a very different graduation of the air-thermometer from what they *assume* as the true one. A more obvious consequence is, that the specific heat of the same mass of air, under a constant pressure, must be independent of the *intensity* of such pressure; and that when a certain mass of air is confined in an inextensible vessel, its specific heat must be independent of the *size* of the vessel. This admits of the most rigorous demonstration, and might even have been inferred from the consideration, that the above *invariable* ratio of the specific heats keeps the quantity of heat which merely raises the temperature quite distinct from what goes to enlarge the volume. Now, the remarkable curiosity is, that in the *Mécanique Celesté*, livre xii., chap. 3, formula are given for expressing the *different* values which the specific heat is fancied to have under *different* constant pressures, and also its supposed *different* values under *different* constant volumes. The same are given by M. Poisson, *Annales de Chimie*, for Aug., 1823, and *Phil. Mag.*, Nov., 1823.

---

* Newton inferred from Boyle's experiments, that the particles repel each other with forces inversely as their simple distances. In this he was quite excusable, it not being then known that a change of density affects the temperature. It is now certain, that whatever be the true ratio, it must be something very different from the inverse of the simple distance; but I think the French philosophers have gone a little too far to the opposite extreme. It is worthy of remark that this important point may be viewed without any regard to the essence of heat.

Sanctioned by such great names, these formulæ must be received as orthodox by those who never think for themselves, or who do not thoroughly examine the investigations from which the formulæ are deduced. As, however, the point is an important one, I shall, for the satisfaction of the rational inquirer, endeavour to put the matter beyond dispute, by giving a very simple demonstration of what I have now alleged, and which is the more satisfactory as it does not require the law of temperature to be known. For, the very same degree being here used for the several specific heats, it may belong to any scale.

Let the temperature of a given mass of air be reckoned on any scale of which the straight line, B E, is a part; and let C F be a line, no matter to our present purpose whether straight or curved, if it be such that any two ordinates, as B C and E F, may everywhere intercept an area B C F E proportional to the quantity of heat which would raise the temperature of the mass of air, under a constant volume, from any point B to any other point E. Let D G be a similar sort of line, so as to make area B D G E proportional to the heat which would raise the temperature of the same mass, if under a constant pressure from B to E. Now, the chief condition on which we proceed is, that the quantities of heat represented by the areas B C F E and B D G E, may be everywhere to each other in a constant ratio, which call that of 1 to k, and which is obviously the same with the ratio of any ordinate of C F to the corresponding ordinate of D G.



Let the point B denote the original temperature of the given mass; raise this temperature from B to E, under a constant pressure; make E e one degree, and draw the ordinate efg.

Then, area E G *g e* is the specific heat, at the temperature E, of the same mass of air dilated by heat under the original pressure remaining constant. Now, the specific heat, when the pressure is constant, is always to the specific heat, when the volume is constant, as *k* to 1; but area E G *g e* : E F *f e* :: *k* : 1; wherefore, E F *f e* would be the specific heat, at the temperature E, were the dilated volume now to remain constant. But since area B C F E is the quantity of heat which would raise the temperature from B to E, under the original volume, and area E F *f e* the heat which would raise it one degree more; therefore, E F *f e* would still have been the specific heat of the same mass of air raised to the temperature E, and all the while under the original volume. Hence, E F *f e* is the specific heat of the same mass of air, at the temperature E, whether under the *dilated* volume remaining constant, or under the *original* volume constant.

By heating up the air from B to E, under the original volume, its pressure is augmented; also, if area E F *f e* were increased as 1 to *k*, it would become the specific heat, at the temperature E, under the augmented pressure made constant. But E F *f e* : E G *g e* :: 1 : *k*, and therefore, E G *g e* is the specific heat of the same mass of air, at the temperature E, whether under the *augmented* pressure remaining constant, or, as in the first case above mentioned, under the *original* pressure constant. The same conclusions would follow, by a similar process, were the temperature E lower than B.

It is thus clearly established, as a necessary result of laying down the *invariable* ratio above mentioned, that neither the *magnitude* of a constant volume, nor the *intensity* of a constant pressure, have anything to do with the specific heat of a given mass of air.

The contrary errors, even if detached or insulated, would have merited correction; but how much more when it is considered that the eminent French philosophers, as already quoted, have built upon these errors an immense fabric of complex formulæ, and have drawn from them a multitude of conclusions!

The theory of rain proposed by the late ingenious Dr. James Hutton of Edinburgh is by many received as the true explication of the phenomenon, viz. that rain is produced by the mixture of masses of moist air having different temperatures. It is known, from experiment, that the variations in the capacity of air, or rather perhaps of space, for moisture, proceed in a higher ratio than the variations on the common scale of temperature, but still more so with reference to the one we were investigating; and therefore, if the mixture had either the mean temperature of the whole, or one still lower, its capacity for moisture would come short of the mean. Hence, a deposition of water will ensue, if the air have been sufficiently moist. There can be no question, that this theory is a possible one, but it would be no easy matter to prove that it is the actual and ordinary mode in which rain is produced. I therefore propose adducing some reasons, countenanced by experiment, which seem to render it very probable that clouds, and rain too, may often be traced to a nearer and more natural source. The Huttonian theory does not readily explain why rain is more commonly preceded or attended with a falling barometer. For it is as easy to conceive mixtures of air occurring whilst the barometer rises as when it falls, and the like objection attaches to the electrical theory of rain. This prognostic did not long escape notice after the Torricellian experiment was made*; and the explanation then given, and for long after received, was, that the air from its rarity was unable to buoy up or support the dense vapours, and so of course down they came. It was not then known that, at the same temperature and pressure, moist air, especially if transparent, is lighter than dry. Yet the observers of that period certainly inferred, on very probable grounds, that there existed a connection between the concomitant circumstances of decreasing pressure and depositions of rain, &c. from the atmosphere. They are further to be commended for seeking a solution in a principle supposed to be known, because reason and facts are always preferable to hypothesis. I am far from reckoning their statical explanation to have no share in the phenomena, though I think there are other and more powerful causes.

---

* I am aware that some of the first observers are said to have coupled rain with a rising or high barometer, and indeed there are exceptions. When the wind is shifting from west to east, the barometer generally rises, though followed with copious rain; and when the wind shifts from east to west, the barometer usually falls, though it remain dry. The motion of the barometer, in these cases, is, to a certain extent, connected with the earth's diurnal rotation.

We are indebted to M. Deluc, *Idées sur la Météorologie*, tom. i. 26, and *Phil. Trans.* 1792, for the first conjectures regarding that theory of the mixture of air and moisture which has since been so ably extended and defended by Mr. Dalton and M. Gay-Lussac, viz. that the quantity of vapour contained in a given space is independent of the presence or density of any other elastic fluid with which it forms no intimate combination; or, that the maximum quantity of vapour which can exist in a given space is the same at the same temperature as it would be did that space contain nothing else. This view of the subject was at first questioned by Saussure, and occasionally since by others, though most of the arguments opposed to it are not much to the purpose. But, as it is pretty well supported by experiments, and is very generally received, I shall adopt it at present as correct. When, therefore, the bulk of a mass of air increases, its entire capacity for moisture should increase at the same rate, were the temperature to remain the same. But when air dilates from a diminution of pressure, its temperature always falls, if there be no accession of heat from some other source*; and this diminution of temperature lessens the capacity of the air for moisture far more than the increase in bulk enlarges it. If, therefore, the air be sufficiently moist, a fall in the barometer should tend to produce clouds or rain.

Such at least would be the result, calculating from the foregoing principles; but the following simple experiment affords a more direct proof, that sufficient rarefaction will always change common undried air into a cloud. Connect a small glass flask, containing such air, with the receiver of an air-pump, by means of an intervening stopcock. Exhaust the receiver, with the cock shut; then, looking attentively at the flask, open it into the receiver, when a momentary mistiness will be perceived in the flask, which is moisture, condensed into a cloud by the cold caused by rarefaction.

---

* A fall in the barometer may not produce a reduction of temperature near the earth's surface. The temperature there may often be preserved, or even raised, by the evolution of the latent heat of the condensing vapour, and by the retention of the heat which formerly escaped upward, but which ceases to do so after the atmosphere is occupied by dense clouds.

The cloud is here formed under peculiar disadvantages, being every where surrounded, at so short a distance, by a warmer vitreous surface throwing in heat upon it. But a cloud which is visible in so small a quantity, would be pretty dense on the large scale. I have never tried this experiment without succeeding; but I believe it may fail, when the air contains little moisture, if the receiver be not large compared with the flask, or if the connecting stopcock have a very narrow bore. By means of an additional orifice and stopcock to the flask, the air might be renewed in it, and the experiment repeated many times, with one exhaustion of a large receiver. I tried a flask with a minute perforation in its bottom, to which I applied my finger, and repeated the experiment more than twenty times with one exhaustion; but this depends on the state of the air. The heat of the hand, however, is unfavourable to the experiment*.

Enough, I presume, has now been said to warrant us to conclude, that when air ascends sufficiently in the atmosphere, it must (being cooled by dilating) constitute a cloud, or, if moist enough, produce rain, &c. For example, if a current of air traverse the ocean till it become very moist; and then, if this current, on arriving at the shore, have to rise higher as the land rises; we have at once the reason why rain usually commences nearer the sea, and extends thenceforward with the wind—why more elevated situations are generally more liable to rain, and also why a wind from the land is more rarely attended with rain on its approaching the shore.

But there is reason to think, that a stream of air, which has been gradually elevated by traversing a rising ground, does not always descend again where the surface does so; but, on the contrary, may, from the upward force it has acquired, even rise higher. In this manner, it may deposit rain whilst rolling far above the tranquil plain, as well as when contending with the asperities of the more elevated surface. If the air over which the raining current passes, be not saturated with moisture, it may absorb or redissolve a part or the whole of the rain descending in it. This is, no doubt, the way in which clouds seem suspended in the air, though in fact they may be falling or floating on with the wind beneath; whilst their place is continually supplied by the successive condensation of other vapour, which, in its turn, is redissolved or swept away by the wind. Much in the same way, clouds are apparently stationed over elevated peaks, or even over large portions of hills; while the fact is, that their particles are moving onward.

---

* I have sometimes been conjecturing whether a hygrometer could be formed on these principles; or, by injecting some additional air into a flask, and then producing a cloud by the cold attending the opening of the flask. The latter method readily produces a dense cloud with a large flask—I have never tried it with a small one. Could the least degree of condensation, which would produce a cloud on opening the flask, be readily ascertained, it might afford the means of estimating the moisture in the air.

The circumstance of clouds frequenting hills, or apparently moving towards them, will admit of a similar explanation, without the aid of an imaginary force residing in hills for attracting clouds and rain. The notion that mountain-caps, or clouds hovering about the tops of hills, are produced by the cooling influence of the summit, seems to me extremely improbable, because such phenomena frequently occur when the air is considerably colder than the surface of the hill; though it is clear they cannot long continue of very different temperatures. But were the colder temperature of the hill really the cause, the cloud would not only touch, but be densest next the surface, wetting it profusely; whereas, the cloud is often observed to be several feet or many yards clear of the hill, and the surface as dry, at least, as that of the surrounding country. A more natural explanation, I presume, is, that the cloud is formed in that part of a current of moist air which is sufficiently cooled by the rarefaction attending its sudden increase of elevation in ascending and rolling over the summit; and that, according as this current afterwards passes on to where it absorbs heat or recovers its temperature by increase of pressure, will it regain its transparency*. The reason why the cloud is frequently clear of the summit, especially during a brisk wind, is, that the centrifugal force due to the curvature of its course over the hill, carries it clear of the summit; and the intervening arched space is left to be occupied by comparatively still air, into which the air cooled by recent rarefaction scarcely enters.

---

* Since such a cloud may apparently remain at rest in the wind, it is manifest that the motion of clouds affords a very doubtful measure of the velocity of the wind.

The origin of the cloud called the *cumulus* may be traced, by means of its horizontal base, to the rarefaction of air. Such a cloud may be situated partly in an ascending portion of a current of moist air, and partly in a descending portion; or it may sometimes occupy the most elevated part of an arched-like sweep of the current. Whenever the air reaches the requisite elevation, it will become opaque, but will regain its transparency so soon as it descends again sufficiently. The opacity will terminate underneath, nearly all at the same level, or in a horizontal plane, if the heat and moisture have been uniformly or proportionally distributed through the air of the current. When the distribution has been unequal, the base will of course be irregular, or may deviate from a horizontal plane. Some other modifications might, perhaps, be accounted for on similar principles.

There is good reason for supposing that air, which has just been suddenly elevated and dilated, is thereby reduced to a much lower temperature than what obtains in air which has remained at that elevation for some considerable time, receiving heat from below, from the sun, or other sources*. Because, the entire fall of temperature due to such dilatation far exceeds a reduction of 1° F. for every 100 yards of ascent. Indeed, the experiment of the cloud in the flask shows that, if an equal weight of air in the upper regions did not contain far more heat than in the lower, the sky would be perpetually obscured with clouds†. Aqueous vapour, by its superior elasticity, would always shoot up through the air to where it would form a cloud, did the temperature decrease as fast as some suppose it to do. It is true, that transparency might still be aided by rapidly descending currents; and it may be partly owing to these, that portions of the sky become visible when the air is loaded with moisture. I am also aware that, in all this, allowance should be made for the different and perpetually varying conditions of the several strata composing the atmosphere.

---

* Moisture which has ascended in the form of transparent vapour, and descended again as rain, &c., must have left its latent heat above. But much heat no doubt moves upward, from its propensity to render the atmosphere of one temperature throughout its whole height, and from the tendency which warmer air has to rise above the colder.

† When the atmosphere is much agitated, and intermixed to a great height, it becomes obscured above, probably from getting colder there, while it becomes warmer and more transparent beneath. In such cases, the clouds often present a very deep blue colour.

A very probable reason why the temperature at the tops of mountains is generally found to be lower than that of air at the same height over the plains, is, that mountains are cooled by, and enveloped in, recently dilated ascending currents of air rolling over them. Snow-clad mountains are, besides, cooled, particularly in dry weather, by the evaporation from the snow. And we may remark, by-the-bye, that, at the same temperature, dry air is, for the above reason, less efficacious in melting snow than moist. Because, the moist air, in place of spending its heat to form vapour, does, in consequence of its touching a colder body, part with a portion of the latent heat of the vapour it already contained, which greatly aids in liquefying the snow. The melting of snow does not, therefore, depend solely on the temperature of the wind, but likewise upon its being previously charged with moisture. When dry air passes over snow on a high mountain, the evaporation, and consequently the reduction of temperature will be greatly promoted by the diminished pressure which obtains at such heights. This may help to explain why *snow winds*, as they are called, should be found so intensely cold. The mere circumstance of wind having passed over a cold mountain, is not a sufficient reason why it should be cold after its descent.

The rise of water, in the water-spout, has been long accounted for in a very rational way. The collision of currents of air from different quarters produce a whirlwind. The air near the axis of rotation is rarefied by the centrifugal force. The pressure on the spot under this attenuated air is necessarily diminished; and, of course, when a whirlwind occurs on the sea, &c., the water rises in the axis, on the same principle as in the common pump. But the rarefied air itself ascends, in virtue of its levity. Its place is supplied by the concourse of the heavier adjacent air, which, in its turn, is rarefied; and, in this manner, an upward current of air is produced, which aids the ascent of the water. Thus far, the explanation is very satisfactory. The other principal part of the phenomenon, the apparent descent of a dense stem from the clouds, nearly over the spot where the water rises, has been ascribed to electricity. But this, in my humble opinion, is rather an evasion than an explanation, and seems nearly allied to the notion that hills attract clouds.

The column which *apparently* descends from the clouds (for any descent is altogether illusory till water actually fall) may be accounted for in the same way as the mountain-cap, viz. that it is moisture condensed by the cold due to the rarefaction which is occasioned both by the whirling motion of the air and by its rapid ascent. The more rapid the rotation, the greater will be the rarefaction and cold; and, of course, the lower down will the condensation take place. The sound and flashes of light seem to be thunder in miniature.

In attending to this and other atmospherical phenomena, most people are embarrassed with a preconception which is not easily overcome—that clouds are solids, or something much more substantial than the air in which they are formed.

Volta supposed that bodies, while passing into the gaseous form, absorb electricity, which they emit again on being condensed. Objections have been made to this theory; but as they rest, in a great measure, on the assumption that we possess perfect electrometers, they do not convince me that the theory is unfounded. Thunder is unknown in the Polar regions, and rarely takes place any where in cold weather. From which it appears, that thunder does not occur in air incapable of containing much moisture; as is further confirmed by its being ordinarily produced in a dense cloud. The conjecture which I have to throw out is, that when a large mass of warm damp air is suddenly moved upward, it dilates, is cooled, and deposits a considerable share of its moisture, which, in laying aside the gaseous form, parts with electricity and emits lightning. The sound may be partly a tremor, which the air sustains at the moment the pressure is relaxed by the vapour losing the elastic form, and may be partly an effect of the electricity in making its escape from the cloud. The thunder and lightning which sometimes attend the condensation of large volumes of steam emitted by volcanoes, are favourable to this theory; as are likewise the noise and lightning of the water-spout, if not some parts of the Northern Lights.

Remarks on Captain Pakenham's *Temporary Rudder, and explanatory of an Alteration proposed.* By Commander JOHN PEARSE, R. N.

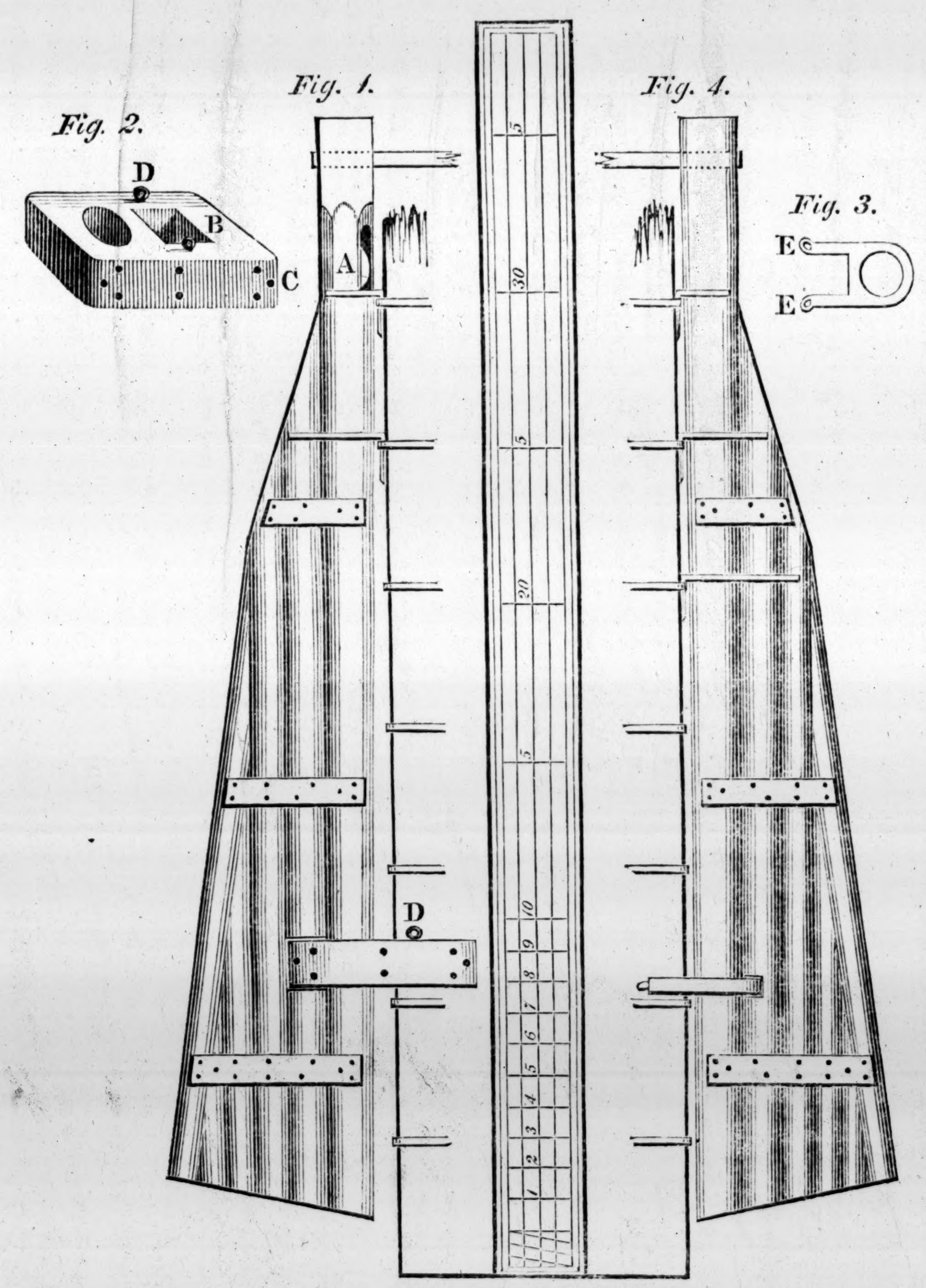
HAVING twice witnessed the application of that most useful invention, it has frequently occurred to me that it may be made much more effective, and the labour and time in making reduced, without lumbering a ship with any additional materials, except something to secure the heel of it in place of the spare lower cap.

The rudder being such an essential part, and requiring to be replaced as expeditiously as possible, in the event of its being damaged or lost by a ship getting on shore, or, which sometimes happens, at sea; and, on such occasions, ships having to depend on their own resources, particularly on long voyages, has induced me to offer the following remarks on it, and an explanation of an alteration I propose.

The top-rope sheave holes being necessarily cut in the spare topmasts, for the expedition of shifting in case of emergency, the mast is so much weakened by it where the most strength is required, when applied as the main piece of a temporary rudder, and twists so much when the ship carries a weather helm, that it frequently becomes necessary to ease it as much as possible by furling the after sails. This defect may be remedied by supplying *one piece* of pine timber of *the length*, and in lieu of *the two* yard-arm pieces which are given to all king's ships as sea store.

Figure 1st. in the accompanying wood-cut, represents the temporary rudder agreeable to Captain Pakenham's plan. By which it will appear evident, that the rudder is considerably weakened by the sheave hole where the greatest strength is required. The diameter of the sheave hole A is one-sixth of the diameter of the mast, and which, from the sheave-hole being through its centre, is reduced in strength and substance full one-fifth.

It also requires considerable time and labour to convert the spare lower cap, to adapt it to the purpose of securing the heel. The wood of the aft-part, at B. fig. 2. requiring to be cut out



to admit of the cap fitting on to the stern-post, and consequently the brace bolts at C. to be driven out, or cut through with a hack saw; the brace bolts at the fore-part also require to be cut, to admit of the corners being rounded off. The bulk of the cap must also contribute to the tendency of the rudder to lift, by the sending motion of the ship in a sea, except it is fitted close under one of the braces, which is not practicable if the pintles are broken and remain in, as is generally the case when a rudder is lost.

From the buoyancy of the materials the rudder is composed of, it is also necessary to append iron ballast to the heel, to sink and facilitate the hanging of it; all of which would be greatly remedied by a cast composition cap, as described in fig. 3. being supplied for the purpose. As its weight would obviate the necessity of appending ballast to the heel, its small bulk would add but little to the tendency of the rudder to lift in a sea; and the time and labour of converting the spare lower cap saved.

The greatest difficulty in hanging it, particularly if there is any motion, is, to conduct the cap to its place. From the position of the eye bolts D, in the spare lower cap, the hawsers which are fastened to them, and which secure it to the stern-post, can be no guide to lead it to its place, consequently it is necessary to have guys to assist it; and which would be remedied by the composition cap I propose, as the hawsers being fastened to the eyes EE would lead it immediately to its place, and obviate the necessity of having so many ropes, which are liable to confuse and get foul of each other.

Line-of-battle ships are allowed only two spare pintles as I have represented in fig. 1. I should propose these being supplied, as it would make the rudder much more secure; and the third brace down, not being much below the surface of the water, can easily be cleared of the pintle if broken and remaining in.

In an eighteen gun-brig, which I was serving as first lieutenant, the temporary rudder was put together and hung in twelve hours. By the alteration I propose, and the superior resources of a large ship, it could no doubt be done in much less time; and it is a much more expeditious means of rendering a ship effective than repairing the pintles, when they are broke and the rudder saved. Accidents, too, may occur, when and where it is absolutely necessary a rudder should be hung before they can be repaired, which, with good smiths, on board a ship, would require several days.

Figure 4th represents the rudder composed of the materials I propose; by which will be seen the difference in bulk of the composition cap and the spare lower cap as represented in fig. 1. It will also be seen, that the alteration I propose strengthens the rudder below, as the space required for its working round the end of the cap in fig. 1. is much larger than in fig. 4.; it also hangs closer to the stern post below.

It is not only king's ships that should be supplied with the means of putting together as effective a temporary rudder as possible, but also Indiamen, and all merchant-ships going long voyages; and having such, I have no doubt two good carpenters in a merchant-ship, would put one together in twenty-four hours.

Plymouth, 14th Feb. 1829.

Analysis of the Mineral Water of Bath.

[Communicated by Mr. A. Walcker.]

SINCE the latest experiments which the springs of this celebrated watering-place have been submitted to by Dr. Scudamore, our attention has been directed, by the masterly analysis of the Carlsbad and some other Bohemian springs by Berzelius, to look out, in the analysis of mineral waters, for several ingredients which were previously not supposed to exist in them. From this consideration, I was induced to subject the Bath water to a new analysis; and, although I wished to spare the reader the perusal of its dull details, yet I think it necessary to state briefly the processes which I have followed, in order to enable him to judge what reliance may be placed in their correctness.

I am indebted for the supply of the mineral water to Mr. R. F. George, M.C.S. of Bath, who was kind enough to have it bottled, with due precautions, under his own inspection, from the pump in the pump-room.

The mineral water, when I received it, was very slightly turbid; the suspended precipitate appeared to consist chiefly of silica, an excess of muriatic acid and application of heat having but very little effect in restoring its transparency. The specific gravity of the water at 60° F. was found to be 1.00202.

In the filtered mineral water, nitrate of silver indicated the presence of chlorine.

When evaporated to $\frac{1}{20}$ of its original weight, and strained off from the precipitate formed, solution of starch and nitrous acid gave no indication of iodine; nor was bromine to be detected by the addition of chlorine; nor nitric acid by boiling it with a gold leaf, and a due quantity of sulphuric acid.

The residue of the water (the carbonates of which had been converted into acetates, in order to prevent the fluosilic acid, if present, from being carried off too rapidly along with the carbonic acid gas) gave no indication of fluoric or fluo-silicic acid, when treated with sulphuric acid in a platinum vessel, covered with a wet watch-glass.

Ebullition proved the presence of carbonic acid in the mineral water, by the precipitation of carbonate of lime.

Muriate of barytes indicated sulphuric acid.

A portion of the mineral water, to which an excess of carbonate of soda was added, was evaporated to dryness, and the residue dissolved in water. A part of this solution was supersaturated with nitric acid, and precipitated with an excess of nitrate of silver. The liquid, separated from the chloride of silver, gave no indication of phosphoric acid, by being duly neutralized with ammonia; nor could any indication of phosphoric acid be obtained, when the solution was precipitated with muriate of barytes, and the fluid strained off from the sulphate of barytes was mixed, after ebullition, with an excess of ammonia. The earthy carbonates obtained in this experiment left silica, when dissolved in muriatic acid. The muriatic solution indicated iron, by the addition of ferro-prussiate of potash. Sulphuretted hydrogen produced in the acid solution a slight precipitate of sulphuret of lead.* Ammonia produced a precipitate, which, when treated before the blow-pipe with a little soda, proved to contain lead; when treated with boracic acid, and a piece of iron wire, no indication of phosphoric acid was observed. Caustic soda, when boiled on the precipitate, took up some alumina. The concentrated solution of the earthy muriates produced no precipitate, when mixed with a saturated solution of sulphate of lime, thus proving the absence of barytes and strontia in the mineral water. Oxalate of ammonia indicated lime; and in the liquid, strained off from the

---

* In a solution which contains at the same time oxide of lead and peroxide of iron, sulphuretted hydrogen does not produce a precipitate of lead, till the peroxide of iron present is reduced to the state of protoxide; and sulphuret of lead, already formed, can be re-dissolved by the addition of a solution of per-chloride of iron. The sulphuret of lead, whenever it is precipitated from a solution which contained at the same time a peroxide salt of iron, appears of a lighter colour, (from the ad-

---

mixture of sulphur produced by the decomposition of the sulphuretted hydrogen,) resembling very much, when a due proportion of iron was present, the precipitate formed by sulphuretted hydrogen in a solution of the per-muriate of tin, for which, on a superficial examination, it may be mistaken; still more so, as the precipitate, its admixture of sulphur being soluble in hydro-sulphuret of ammonia, greatly disappears by the admixture of this re-agent. Muriatic, sulphuric, and nitric acid diminish the delicacy of the sulphuretted hydrogen as a test for lead. In a solution containing $\frac{1}{500000}$ of its weight of the acetate of lead, the effect of the re-agent is still perceptible when the diameter of the fluid under examination amounts to about three inches: $\frac{1}{200000}$ of the acetate is still indicated by the test, the diameter of the fluid being about one inch; and in a solution containing $\frac{1}{15000}$ of the acetate of lead, the indication by the sulphuretted hydrogen is very distinct, when the diameter of the fluid is but $\frac{1}{8}$ to $\frac{1}{6}$ of an inch. But if a solution of the concentration last mentioned contains about $\frac{1}{15}$ of its weight of muriatic acid, the change of colour, which the sulphuretted hydrogen produces, resembles that which would take place in a solution containing but $\frac{1}{200000}$ of the salt of lead. When the muriatic liquid is neutralized by carbonate of soda, the re-action appears with its full intensity. This is not quite the case when ammonia has been used for neutralization. Sulphuric and nitric acid act in a similar way; the former, however, even if present in a still smaller proportion. It is not the decomposition of the sulphuretted hydrogen which diminishes the efficacy of this test; (as one might suspect the muriatic acid to contain some chlorine, and both the other acids to contain some nitrous acid;) for the excess of the sulphuretted hydrogen present is proved, as well by the smell as by the copious precipitate, which an additional quantity of the salt of lead produces.

The sulphuret of lead already formed cannot be re-dissolved by muriatic acid. Acetic acid does not diminish the effect of the test in any sensible degree; nor does chloride of sodium, even if the liquid contains $\frac{1}{5}$ of its weight. Muriate of ammonia, however, in a similar proportion, appears not to hinder the full effect of the test in the first moments; but the reaction produced becomes gradually less and less intense, although the fluid continues to contain an excess of sulphuretted hydrogen. When the solution of the lead is less concentrated, a smaller proportion of the acids was, of course, required to produce the same effect. The sulphuret of lead, precipitated from a solution containing an excess of the above acids, differs very much in its appearance from that where no acid is present; the former being of a denser aggregation, and a bluish black colour; the latter, blackish brown.

oxalate of lime, phosphate of ammonia, together with pure ammonia, indicated the presence of magnesia.

The earthy carbonates, as well as the residue of the water*, gave no indication of manganese, when treated before the blowpipe, either with borax and nitre, or with soda.

Turmeric paper was not acted upon by the mineral water, even when concentrated by evaporation. Slightly reddened litmus paper turned blue, after some time, by the influence of the earthy carbonates. Tincture of galls proved, by want of re-action, that no iron was kept in solution.

A portion of the dry residue of the water was heated to redness. The ignited mass was dissolved in boiling water; and from the filtered solution, the sulphuric acid and the magnesia (of which a small quantity was found to be dissolved) were precipitated with muriate and hydrate of barytes. The boiling liquid, strained off from the precipitate, was evaporated to dryness, and the dry residue dissolved in a weak solution of carbonate of ammonia. The ammoniacal solution, after separating the last traces of lime by oxalate of ammonia, left, after evaporation and ignition, alkaline chlorides, one portion of which was dissolved in water, together with an adequate quantity of phosphate and carbonate of soda, and evaporated to dryness. The saline residue, by its perfect solubility in water, proved the absence of lithia in the mineral water. Another portion was dissolved in water, together with three times of its weight of per-chloride of platinum, and evaporated to dryness. The residue, when dissolved in spirit of wine, left some potassio-chloride of platinum. Thus the presence of potash and soda in the mineral water was evinced.

The constituents of the Bath water, according to the foregoing experiments, are therefore—chlorine, sulphuric acid, carbonic acid, potassa, soda, lime, magnesia, oxide of iron, alumina, and silica. Besides these, the mineral water contains some extractive matter: its residue, when evaporated, being coloured, and containing an admixture of carbon, after ignition. The relative proportions of these ingredients were ascertained in the following manner:—

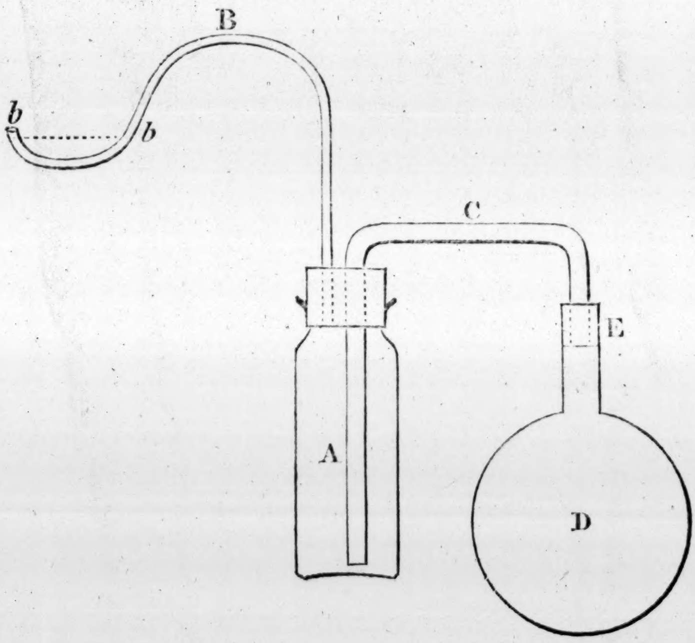
---

* The residue, when heated before the blowpipe, with an addition of carbonate of soda, does not cover the charcoal with a sublimate of oxide of lead, on account of the traces of this metal being defended from volatilization by the large quantity of the foreign substances inclosing them. The fused mass, however, left in the mortar, after a careful elutriation, some particles of metallic lead, even though but one or two grains had been employed for the experiment. The presence of lead in the residue may easily be ascertained also, by digesting on it from fifteen to twenty times of its weight, of a solution of acetate of ammonia, with some additional acetic acid. The fluid filtered off from the earthy residue is precipitated by sulphuretted hydrogen.

I.—14.47 cub. inch. of the mineral water were boiled in a suitable apparatus*, till the whole quantity of its gaseous contents

---

* Having frequently occasion to determine the gaseous contents of mineral waters, the following little apparatus has proved to me, by frequent experience, to be a very useful one for that purpose.



A is a narrow cylindrical vial, containing from 2½ to 3 cubic inches. Into its aperture a cork is thrust, and fastened down by a wire. (The lower surface of cork is well imbibed with a solution of mastic and turpentine in spirit of wine, in order to prevent its swelling, and consequently changing the bulk of the interior of the bottle.) C is a glass tube, of ⅛ to ⅙ inch bore, to be connected to the glass ball D, the neck of which is cylindrical, (if it widens towards its mouth, the cork is easily forced out when the water boils,) and its flat ground aperture of such wideness as to be easily closed by the finger. B, the tube for carrying off the gas, is of no more than about $\frac{1}{20}$ inch bore. When the apparatus is to be used, the bend *b b* of the tube B is first filled with mercury, by dipping it into that metal; then the cork E is thrust into the neck of the glass ball, entirely filled with the mineral water, by which process part of the water is forced into the tube C, and the air in the vessel A compressed in some degree. By holding the whole contrivance dexterously, with the bend of the tube B upwards, the column of mercury is sufficient to was expelled. The gas collected over mercury was partly absorbed by a solution of caustic soda, and was found to consist, in one experiment, of 0.41 cub. inch. carbonic acid gas, and 0.29 cub. inch. residue (not absorbable by the alkali) in a dry state, (deduction being made for the elasticity of the aqueous vapour,) at 60° therm. and 30″ bar. In another experiment, 0.38 cub. inch. carbonic acid gas, and 0.28 cub. inch. residue, were obtained, the mean of which gives, for one pint of the mineral water, (34.659 cub. inch., or 8767.7 grains,) 0.95 cub. inch. carbonic acid gas.

Twelve cub. inch of the mineral water, mixed with a sufficient quantity of diluted sulphuric acid, gave, when boiled till all disengagement of gas had ceased, 0.89 cub. inch. carbonic acid gas; which quantity gives, for one pint, 2.57 cub. inch. Thus one pint of the mineral water was found to contain 0.95 cub. inch. dry carbonic acid gas, at 60° therm. and 30″ bar., in a free state, and (2.57 – 0.95) 1.62 cub. inch. = 0.75787 grains carbonic acid gas, united to the bases: making, for 1000 grains of the mineral water, 0.08609 grains carbonic acid.

prevent any gas from escaping. By the first effect of heat applied to the ball, part of the mineral water, which is afterwards boiled by the steam passing through it, is carried over into the vessel A. Boiling is to be continued till the increase of gas in the measuring tube has perfectly ceased. The advantages which this contrivance offers are—1. That the whole quantity of the mineral water under examination is submitted to continued ebullition, (no part of it being carried over into the measuring tube.) 2. That the quantity of water condensed in the measuring tube is, proportionately, very small. The air of the vessel A is so perfectly expelled, that the apparatus fills almost perfectly, (leaving, perhaps, an air-bubble, of the size of a large pin's head,) when the apparatus is allowed to cool, with its tube B plunged into hot water. The cubical contents of the bottle A, with its pipes, are to be deducted from the volume of the gas obtained, and correction to be made, when the air in the bottle was of another temperature or density. In order to empty the little apparatus, when the experiment has been finished, it is best to connect it, when still hot, with its top downwards, to a common pair of bellows, and to drive the water out by a current of air. The air of the lungs is, of course, not proper for that purpose. Besides the little effect which it would have in drying the bottle, it would leave it partly filled with carbonic acid gas. The balls which I make use of, contain from about 7 to 12 cub. inch.; and I obtain from the smaller quantity, when the mineral waters are rich of gas, results which correspond as closely as can be expected, when the experiment is repeated. If the gas, not absorbable by the caustic alkali, is the object of research, I used to fill the little apparatus with pure carbonic acid gas, and collect the gas in the eudiometer itself, into which a solution of caustic potash has been injected before.

The residue, not absorbed by the caustic soda, gave, by detonation over mercury, 18.2 per cent. oxygen in one experiment—in another, 18.6 per cent.; and may be considered as atmospheric air, originally dissolved by the water, and partly decomposed by its protoxide of iron.

II.—56000 grains of the mineral water were mixed with an excess of carbonate of soda, and evaporated to dryness. The residue was perfectly lixiviated with water, till all reaction on the test paper had ceased; and the saline solution was again evaporated, and kept in fusion over a spirit-lamp for some time, in order to decompose the soda-carbonate of magnesia which might have been formed. The fused mass, when lixiviated with boiling water, left some earthy carbonates, which were added to those obtained by the first lixiviation.

III.—One-third of the solution obtained in II., when supersaturated with muriatic acid, and precipitated with muriate of barytes, gave 76.394 gr. of sulphate of barytes; making, for 1000 grains of the mineral water, 2.48539 gr., equal to 0.85471 gr. of sulphuric acid*.

IV.—Another third of the solution, supersaturated with nitric acid, and precipitated with nitrate of silver, gave 20.442 gr. of chloride of silver; making, for 1000 gr. of the water, 1.09511 gr., equal to 0.27017 gr. of chlorine.

The remaining third, being neutralised with muriatic acid, was evaporated to dryness, and re-dissolved in water, in order to try if any silica had been dissolved by the carbonate of soda; which was, however, not the case.

V.—The earthy carbonates obtained in II. weighed, after ignition, 57.757 grains†; 56.8 gr. of which, dissolved in muriatic acid and evaporated to dryness, left, when first macerated with muriatic acid, and afterwards perfectly lixiviated with water, 2.539 gr. of silica; which quantity, if proportionated to the whole amount of the residue, gives, for 1000 gr. of the water, 0.04610 gr. of silica.

---

* The equivalent numbers are computed according to Berzelius's latest experiments and computations. Vide his Annual Report to the Academy of Sciences of Stockholm for 1827.

† Wherever the nature of the precipitate allowed it, the filter was burnt, and the weight of its ashes deducted from the weight of the ignited precipitate. When the precipitate was to undergo a further examination, as much as conveniently could be done was taken from the

VI.—The muriatic solution obtained in V. was precipitated by sulphuretted hydrogen. After twenty-four hours, the liquid was strained off from the sulphuret of lead*; and the hydrothioic acid being expelled by boiling, the iron was again oxidised by chlorine. The liquid (a solution of muriate of ammonia being added) was now precipitated in a closed vessel, by ammonia. The precipitate was kept for further examination.

VII.—Half of the liquid separated from the precipitate in VI. was precipitated by oxalate of potash. The oxalate of lime, when converted into sulphate, weighed 37.720 gr.; which quantity, proportionated to the whole amount of the earthy residue, gives, for 1000 gr. of the water, 1.36984 sulphate of lime, equal to 0.56894 gr. of lime.

VIII.—The liquid separated from the oxalate of lime was precipitated by ammonia and phosphate of ammonia. The ammonia-phosphate of magnesia, after washing with water containing from three to four per cent. of ammonia†, gave, after ignition, 6.139 gr. phosphate of magnesia; which, com-

---

filter and ignited; then the filter, together with the remainder of the precipitate, was burnt and ignited; and the weight of both portions was separately ascertained.

* The filter containing the sulphuret of lead was burnt, and the ashes (with an addition of carbonate of soda) being reduced before the blowpipe, the fused mass, taken from the charcoal, left, by a careful elutriation, particles of metallic lead in the mortar, which, when collected, amounted to 0.032 gr. I assume the lead to originate from the pipes or pump used for the conveying of the water; and although its quantity is very trifling, yet it deserves the attention of the managers of the Pump-room, whether a more proper material for that purpose might not be selected.

† When distilled water alone is used for washing, the precipitate begins to dissolve as soon as the saline liquid is strained off, and the washing fluids are precipitated, as well by ammonia as by the saline fluid first separated.

puted for the whole amount of the residue, make, for 1000 gr. of the water, 0.22294 gr., equal to 0.08175 gr. of magnesia.

IX.—55000 grains of the mineral water were evaporated. The residue, dried at a temperature some degrees above 212°, (where, however, a dissipation of muriatic acid already took place,) weighed 114.75 gr. making for 1000 gr. of the water 2.08636 gr. It was afterwards ignited for some time, after all disengagement of muriatic acid had ceased, (in which state it weighed 110.55 gr.,) and then boiled with water and an excess of pure quicklime, in order to decompose a small portion of magnesia salt, which the mass was found to contain. The liquid was strained off when boiling, and the earthy residue on the filter was perfectly lixiviated with boiling water. The fluids obtained were precipitated, first, by muriate of barytes, then by carbonate and oxalate of ammonia. The fluid, when separated from the precipitate, gave, by evaporation and fusion, 26.46 gr. of the chlorides of potassium and sodium, which, when dissolved in water, left 0.15 gr. earthy residue, thus leaving 26.31 gr. of the chlorides for 55000 gr. of the water.

X.—The solution of the chlorides was mixed with three times of their own weight of per-chloride of platinum, dissolved in water. The mixture was evaporated to dryness, and the residue macerated first with 38 times of its own weight of alcohol, sp. gr. 0.84 at common temperature, (the quantity which I found the *crystallized* sodio-perchloride of platinum to require for solution at 60° temp.) The alcoholic solution being decanted, the residue was boiled with a new portion of alcohol. The insoluble potassio-perchloride of platinum being separated, washed with alcohol, and dried at 212°, amounted to 6.378 gr. equal to 1.96130 gr. of chloride of potassium, which quantity gives for 1000 gr. of the mineral water 0.03566 gr. equal to 0.02256 gr. of potassa.

The quantity of chloride of sodium obtained from 1000 gr. of the mineral water amounts thus to (26.31—1.9613) 0.44270 gr. equal to 0.23591 gr. 55 of soda.

XI.—The slight precipitate, adhering to the sides of the bottles which the mineral water had been contained in, was dissolved by muriatic acid. The solution diluted with the rest of the mineral water left in the bottles (1535 gr.) was digested on the earthy residue obtained in IX. The acid solution was evaporated to dryness, and re-dissolved in muriatic acid and water. Filtered off from the remaining silica, it was precipitated by ammonia. This precipitate, together with the precipitate obtained in VI. were dissolved in an excess of muriatic acid, and precipitated by bi-carbonate of ammonia. The weight of alumina and peroxide of iron thus obtained from 112535 gr. of the mineral water, was found to be, after ignition, 0.499 gr., 0.459 of which, when dissolved in muriatic acid, and decomposed by caustic soda, left 0.241 gr. of peroxide of iron, leaving for the alumina 0.218 gr.

By proportionating these quantities, we obtain for 1000 gr. of the mineral water 0.00215 gr. of alumina, and 0.00237 gr. of per-oxide of iron, equal to 0.00213 gr. of the protoxide.

Thus, recapitulating the foregoing results, the solid ingredients contained in 1000 gr. of the Bath water amount to—

Potassa . . .	0.02256
Soda	0.23591
Lime	0.56894
Magnesia . . .	0.08175
Protoxide of Iron . .	0.00213
Alumina . . .	0.00215
Silica . . .	0.04610
Carbonic Acid . .	0.08609
Sulphuric Acid . .	0.85471
Chlorine . . .	0.27017
	2.17051
From which are to be deducted .	0.06104
As the equivalent of oxygen for 0.27017 of chlorine, leaving . . .	2.10947 gr.

which quantity coincides nearly with 2.08636 gr., the weight of the dry residue obtained from 1000 gr. of the mineral water, which had, however, lost part of its muriatic acid.—(IX.)

If we arrange the above ingredients into binary combinations, according to the predominant chemical affinities, (in which state they will exist in the mineral water, at least in the greatest proportions,) we find the Bath water to contain—*

	In 1000 gr.	In 1 pint, (34.659 cub. inch.)
Chloride of Sodium . .	0.21560	1.89031
,, Magnesium .	0.19018	1.66744
Sulphate of Potassa .	0.04173	0.36588
,, Soda . .	0.27618	2.42145
,, Lime . .	1.16371	10.20303
Carbonate of Lime . .	0.15208	1.33339
Proto-carbonate of Iron .	0.00347	0.03042
Alumina . . .	0.00215	0.01885
Silica	0.04610	0.40419
Extractive matter . .	,,	,,
	2.09120 gr.	18.33496 gr.
Carbonic Acid Gas } At 114° temp.	. .	0.05 cub. inch.
Atmospheric Air } At 114° temp.	. .	1.74 cub. inch.

In distributing the acids and bases, there remains a surplus of 0.01827 of carbonic acid. The processes by which the different bases were separated being more complicated, it is to be expected that the loss which they sustained is proportionably greater than with the acids. Besides, the determination of the carbonic acid itself is less certain than that of the other constituents, on account of the comparatively small quantity employed for the experiment, and the simplicity in transferring measure into weight.

The re-action which tincture of galls produces in the mineral

---

* The results of this analysis differ, at the first appearance, very much from those obtained by Dr. Scudamore, the cause of which is the different theoretical view which the Doctor has followed in computing the binary combinations. Having transferred the data of Dr. Scudamore into equivalents, according to the greatest mutual affinities, I subjoin a table of his analysis, computed for the same quantity of the mineral water (34.659 cub. inch.), in order to facilitate a comparison.

	1 pint.
Chloride of Sodium . .	1.53
,, Magnesium . .	1.92
Sulphate of Soda . .	0.19
,, Lime . .	12.29
Carbonate of Lime . .	0.66
Proto-carbonate of Iron . .	0.025
Silica	0.24
	16.855

water, just drawn from the pump, is slighter than we could anticipate it to be, according to the proportion of iron found by the analysis: the cause of which, however, is, that a part of the iron is precipitated instantaneously, when the mineral water, by pumping and pouring into the tumblers, mixes with the atmospheric air. It is therefore merely suspended in a very minutely divided state, (probably in the form of a silicate,) and has no effect on the re-agent, which is only acted upon by that portion of iron which is present in a state of solution.

Analyses of two Mineral Springs in Windsor Forest.

Captain F. Forbes of Winkfield Place, Windsor Forest, discovered, some time ago, two mineral springs on his estate; one, the analysis of which is stated under A., in the immediate neighbourhood of his mansion; the other, mentioned hereafter under B., at some distance. Both these mineral springs, belonging to the magnesio-saline class, have since been used, as I am informed, by a great number of patients; and the good effects which have been observed from their use have induced Captain Forbes to build a pump-room for the accommodation of the public.

The mineral waters which were sent to my laboratory for chemical examination were colourless, almost perfectly transparent, and without smell. If exposed to the air in an open vessel, they very soon covered the sides of the glass with a precipitate of carbonate of lime. Boiling produces a very copious earthy precipitate. Slightly reddened litmus paper is turned by them into blue; the carbonic acid gas, which they yield by boiling, must therefore be considered entirely to exist in them in union with the earthy carbonates, forming bicarbonates.

Both mineral waters were examined in a similar way to the Bath water; and the results of their analysis gave their constituents as follows:—

	1 pint (36.459 cub. inch.) of the mineral water.	
	A.	B.
	contains	
Carbonate of Lime	6.0630	8.2507
Sulphate of Lime	9.8904	8.3064
,, Potassa	1.3549	1.1382
,, Soda	15.5779	17.1761
,, Magnesia	20.8704	21.1920
Nitrate of Magnesia	2.6551	traces
Chloride of Magnesium	19.6909	26.3169
Silica	0.5033	0.9210
Alumina	0.5721	0.3938
Extractive matter	,,	,,
	77.1780 gr.	83.6951 gr.
Carbonic Acid Gas at 51° at the temp. of the wells.	2.786 cu. in.	3.306
Atmospheric Air	0.611 cu. in.	0.658
Specific Gravity, at 60° Fah.	1.00737	1.00897

Adolphus Walcker.

German Spa, Brighton,
Feb. 28, 1829.

A Letter to Mr. Baily, *admitting his* un*disputed Claim to a Theorem relating to* Survivorships. By a Correspondent.

Dear Sir,—I have been thinking, that, as an attempt has been made to direct the public attention to the oversight which I acknowledged to you the moment that I was aware of it, it might be right that I should ask you, whether there was any form, or any channel, by which you would like that I should publicly admit your undisputed claim to the discovery which I have lately printed in capitals as my own: and to assure you, that you cannot be more willing to point out such a proceeding than I should be to comply with your wishes.

I have certainly given you an advantage, in printing, as a conspicuous part of my paper, a remark which you had only thought worthy of being inserted in a note; and for myself, the circumstance has been a little unlucky, not from any censure that may have been passed on me, as not having read every note even in the best book relating to the general subject of my essay; and still less from the contemptible suspicion of my having made a childish attempt to deck myself in borrowed plumes, and then to hold them so high that the slightest breath would blow them away: but because the occurrence tends to divert the attention of a reader from the essential subject of my essay; which, you may have observed, is, first to establish the superior convenience of *a good formula* in *preference to all tables* formed from a limited observation, for all ordinary cases of the valuation of annuities; that is, between the ages of ten and seventy: and, secondly, to show, as I may hereafter do still more fully, that *a uniformly increasing decrement of survivors*, throughout the middle of life, will afford a value of mortality sufficiently near to the results of tables the most discordant among themselves, provided that the rate of increase be properly adjusted to the table.

I am, dear Sir,
Your faithful and obedient servant,
Waterloo Place, 1 *Feb.* 1829. * * * *

On the Atmospheric Influence of Woods.

THE effects produced on climate, by the preservation or destruction of forests, is an interesting subject of inquiry, which has sometimes engaged the attention not only of philosophers but of legislators. In France, about forty years ago, some alarm was created on account of the rapid cutting down of woods which had then changed owners, or were less carefully protected than they had formerly been. Where wood forms a great part of the fuel of a country, a strong motive will always exist for the planting of trees, and for guarding against their wasteful removal. But another evil than the deficiency of firewood, was then apprehended. It was contended by Cadet de Vaux, and other writers, that the great droughts experienced in some districts, were caused by the absence of the trees, which used to attract the clouds, and thus conduct to the earth those fertilizing showers, which reward the labours of the husbandman with an abundant harvest. Amidst the agitations of the Revolution, the complaints of these writers were listened to, and their representations were more than once brought under the consideration of the Convention and the Council of Five Hundred. We do not precisely recollect what legislative measures were adopted; but we believe, among other things, the sale of the National Forests was stopped. However, it does not appear that what was then done, though it may have saved some of the property in the possession of the state, had any important effect on the whole. Indeed it will be seen from what follows, that since that period, the woods of France have sustained a considerable diminution, equal in extent to the difference between $\frac{1}{7}$ and $\frac{1}{12}$ of the territory.

In the mean time, the investigation of this interesting subject has not been neglected. The attention of naturalists and men of science in the Netherlands was, in the course of last year, called to the alterations produced in the physical state of countries, by the destruction of forests, in consequence of a prize for the investigation of this important question being offered by the Royal Philosophical Society of Brussels. The essay which obtained the prize, was written in French by M. A. Moreau de Jonnes, a staff-officer in the army. It appears that the author does not altogether concur with M. Cadet de Vaux; but we have not seen the original, and the following account has reference to a German translation, by M. Widenmann, Professor of Natural History, at Tubingen. In Germany, where great importance is attached to the subject, the work of M. Moreau de Jonnes has been, upon the whole, very favourably noticed by the reviewers of that country, and we have taken the liberty to make use of some of their observations.

The author begins with a statistical account of certain woods and forests, from which, though he complains of the insufficiency of the documents available for his calculation, at least one conclusion, which appears pretty well founded, may be drawn. According to the data here made use of, the woods in France amounted, in 1750, to more than a fourth of the surface of the whole country; in 1788, to a seventh; and in 1814, to not quite a twelfth of that surface. Thus, within sixty-four years, 5000 square miles of the woods of France must have disappeared. In England, according to the author's estimate, the woods amount to only one twenty-third of the surface.

CHAP. I.—*Influence of Woods on the Temperature of Countries.*

Woods lower the temperature. In the author's opinion they have this effect, because, on account of their dark colour, they reflect back few rays of light and heat to the atmosphere, and because they keep the soil damp, and therefore render more heat necessary to promote evaporation. But whether the difference of the mean temperature between Rome and Massachusetts, which, in the same geographical latitude, amounts to 6⅓ cents of a degree, is to be attributed almost entirely to the woods, does not appear so easily demonstrable. That woods on mountains have a great influence in intercepting and beating down vapours is very probable; but the author's assertion that the increased condensation of vapours has for its consequence a greater absorption of heat, is unsupported by proof. It must also be observed, that though woods diminish temperature, and though the author proves this fact by practical observations, yet the experiments as cited by him are so detached, as to leave room for considerable objection. Vienna and Troyes have not a difference of temperature, merely because eastern countries are generally more covered with wood, but also because the site of Vienna is 250 feet higher, and lies nearer high mountains than Troyes. Again, Berlin is colder than Leyden in Holland, not altogether in consequence of Brandenburg being a woody country, but also because it is exposed to the cold east wind. The translator points out the difference of height, which in the original is unattended to.

The author shews from historical comparisons, that the clearing away of woods makes the temperature of countries warmer. Whether the proofs that he adduces for this purpose are all quite convincing, we cannot positively affirm, though the fact itself is true; for France is no more deprived of its woods than England. If, then, there be, at present, nearly the same degree of heat in London and Paris, while, in the time of Tacitus, Gaul must have been colder, it is not quite clear why this contrary influence should proceed from the same cause. But it is also doubtful whether Tacitus would not even now, if he had no assistance from the thermometer, call London milder than Paris, because severe winters occur less often in the former than in the latter city. But though the details of these comparisons may occasionally be uncertain, and more particularly those which relate to the question, whether a difference of temperature in former times ought to be attributed solely to the great abundance of woods and forests, yet a collection of the accounts of a greater degree of cold having heretofore existed, is doubtless very important. There can be but few to whom it will not be interesting to learn how much the cold of winter, especially in the south of Europe, has diminished.

CHAP. II.—*Influence on the quantity of Rain.*

The author here brings forward some observations to prove that more rain falls on the sea-coast than in inland districts, and that, moreover, when chains of mountains run parallel to the sea-shore, the sides next the sea receive more rain than their opposite sides. In reference to woods, however, the author supposes that he may lay it down as a fact confirmed by observation that wood-lands in flat countries do not perceptibly increase the quantity of rain; but that woods on mountains have a perceptible influence in producing that effect. He thereupon founds the conclusion, that if mountains are planted with trees, the quantity of rain in their vicinity will be increased, and that the progressive diminution of rain in the South of Europe is to be ascribed to the destruction of the mountain woods. But the author does not appear to have been sufficiently careful in ascertaining what are the places in which the rain has diminished. It has not diminished in all parts of the south of Europe; for Flaugergues has found that the quantity of rain has considerably increased at Viviers, in the south of France, since 1778. The Milanese Ephemerides indicate the same thing for Milan, and all the assertions of this kind require further demonstration.

CHAP. III.—*Of the Influence of Woods on the Humidity of the Atmosphere.*

Here the author describes among other things an interesting experiment with the hygrometer, according to which the humidity of the air in the West Indies is found to be expressed by the numbers 3, 4, and 15, according as the observations are made on the coast in the midst of cultivated plantations, on the borders of mountain woods, or in the midst of those woods. There seems, however, to be some obscurity in an observation which the author is induced to make, namely, that the humidity of woods in the torrid zone extends *far* above the extremity of the scale. Now, as the hygrometer is usually graduated up to the point of complete humidity of the atmosphere, it can only be said that the moisture is precipitated in greater abundance than is necessary to bring the hygrometer to the highest degree.

We cannot venture to quote any more of these observations, or to explain the grounds of the doubts to which the conclusions drawn by the author give rise. But notwithstanding these doubts, we confess with pleasure that much information is to be found in the work. At the same time we must regret that the translator has not given to his version that great superiority over the original which it would have obtained had he subjected several of the author's statements, such as those relative to the comparative humidity of the Mark of Braudenburg and Holland, to critical investigation.

CHAP. IV.—*Influence of Woods on Springs and Running Water.*

That countries, especially mountainous countries, which are covered with woods, also abound more in waters than others, is a fact which may be asserted with little fear of contradiction.

CHAP. V.—*Of the Influence of Woods on the Wind and on the State of the Atmosphere with respect to Health.*

Though many remarks which occur in this chapter are just, we are much surprised at some of the assertions, and we think we do not err in considering them unfounded. Among these is the assertion that the impetuosity of the winds, where there are no woods to mitigate its violence, has rendered a great part of Great Britain barren. If the author's estimate, that the waste lands amount to $\frac{22}{73}$ of the whole surface of Great Britain be true, it does not follow that these lands are barren in consequence of a deficiency of trees. The heaths in the north of Germany, which are not all situated in places entirely destitute of woods, shew clearly enough that circumstances, which accompany an effect, are not always those which produce it.

The comparison between that part of Tartary inhabited by the Calmucks and Lombardy, appears to be equally unfounded. Whoever in this instance, though the geographical latitude should be the same, expects to find the climate in both regions alike, and ascribes the dissimilarity of the climate to the want of trees in Tartary, must certainly have allowed many circumstances, which ought to have been taken into account, to pass unnoticed.

CHAP. VI.—*Influence of Woods on the Fertility of the Soil.*

We also meet with many remarkable observations and important conclusions in this chapter. But upon the whole, we think that this essay must be considered as a work which has not been reflected upon with sufficient deliberation. Nevertheless, it contains an abundant collection of curious facts; and though some of these facts are not well applied, and the accuracy of others remains to be proved, the book will, in the mean time be found an excellent contribution towards the explanation of the subjects of which it treats.

There are occasionally some obscurities in the reasoning; but whether these ought to be attributed to the author or the translator, we have not at present the means of determining.

Additional Remarks on the Stowage of Ships. By Commander JOHN PEARSE, R. N.

MY former remarks were written with the intention of endeavouring to shew that we have no regular system for stowing our ships, and that an alteration in the distribution of the weights is necessary. Subsequent to their publication, I have seen an authentic calculation of the weight and pressure, according to the ordinary distribution in a modern 74-gun ship. This has induced me to publish these additional remarks, by which I shall be enabled to prove that the alterations I proposed would materially contribute to remedy the inequality of the weight and pressure represented in the calculation alluded to; and which is referred to in the following observations of Dr.

Young's, in his remarks on the employment of oblique riders. *Philos. Trans.* 1814.

"It is unnecessary to explain here the well-known inequality of the distribution of the weight and pressure, which causes almost all ships to have a tendency to arch or hog, that is, to become convex upwards, in the direction of their length. It is possible that there may be cases in which a strain of a very different nature is produced; but in ships of war this tendency appears to be universal.

"It is, however, very different in degree in the different parts of a ship; and, of course, still more different according to the different modes of distribution of the ballast and stores, which may occur in different ships; but, in ordinary cases, it will probably be found nearly such as is represented in the calculations subjoined in the note*, deduced from data, which have been obligingly furnished by an acute and experienced member of the Navy Board."

By the above calculation, it will appear that there is an excess of weight at both extremities, and that in the adjoining sections the pressure greatly preponderates. Consequently, these forces are opposed to each other, and in a direction very prejudicial to the ship; as it is not only the evil of weight preponderating at the extremities, the power of which will be increased by the action of the ship, but from the formation of the body at those parts they can afford but a feeble resistance to so great a force.

Perhaps the following figures will best explain where the weights and pressure now preponderate, what particular weights may be supposed to cause an excess, and such as may be most conveniently and advantageously transferred.

---

* "In a modern 74-gun ship, fitted for sea, the length being 176 feet, the breadth 47½, the the forces are thus distributed.

Aftermost	49 feet	Weight 699	Pressure 627	Difference 72 tons.
Next	20	297	405	— 108
	50	1216	1098	118
	20	290	409	— 119
	37	498	461	37
	176	3000	3000	000

Fig. 1.

Excess of Weight 72 Tons.	20 Feet.	Excess of Weight 118 tons.	20 Feet.	Excess of Weight 37 tons.
49 Feet.	108 tons Excess of pressure.	50 Feet.	119 tons Excess of pressure.	37 Feet.

Fig. 2.

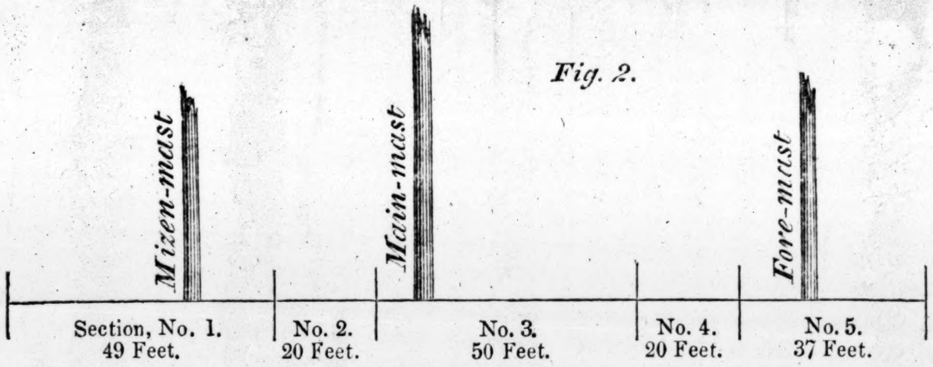


Figure the 1st represents the length of a 74-gun ship, divided into five sections, and the excess of weight or pressure in each.

Figure the 2nd also represents the length, divided into sections, and the positions of the masts, by which it will be seen that they are situated in those parts where the weights preponderate; and that they greatly contribute to it is a very natural conclusion.

In the foremost section there is, unavoidably, not only the weight of the foremast and its rigging, but the bowsprit and anchors also; the latter stowed at the extremity of the section, and the bowsprit projecting considerably beyond it; which, on the lever principle, assisted by the action of the ship and the excess of weight, and being situated in a part which from its formation affords but little resistance, and which is frequently left unsupported, must considerably augment the pitching motion, and contribute to the tendency to arch or hog.

In the aftermost section, the mizen-mast may be considered as contributing to the excess of weight; but it is not of such magnitude as the fore or mainmast; and its position is much more favourable for the ship than the foremast. The strain also on the after part is not so great as forward, as it is seldom left so entirely unsupported by the passing of a sea, and the sending motion is not so sudden or violent as the pitching.

In the extreme sections, and from the formation of those parts below the surface of the water, the upper works may also be considered as contributing to the excess of weight.

In the middle section, the mainmast, from its magnitude, may be considered as materially contributing to the excess of weight; and that, according to the ordinary distribution of the weights in the main hold, a large proportion of it may be supposed to fall in the vicinity of the mast. If this conclusion be admitted, it must appear prejudicial, and the more so, in consequence of the pressure preponderating so much at both extremities of the section. Among other weights concentrated about the mast is the shot locker, and which must considerably add to the unavoidable and too great weight of the mast alone acting on so small a space.

I shall now endeavour to explain, how far the alterations proposed in my former remarks would be likely to remedy the inequality of the distribution of the weight and pressure specified in the calculation.

In the foremost section No. 5, where there is at present an excess of weight, are placed the bower anchors, and the gunners', boatswains', and carpenters' sea-stores, which I proposed transferring; the anchors further aft, by removing the catheads to the fore-parts of the channels, which, although by the removal still situated in the same section, would considerably lessen the power of their weight on the ship in a sea; and the sea-stores to the after cock-pit, which will be found situated in section No. 2, where the pressure now preponderates.

In the after section No. 1, where there is also an excess of weight, the bread is stowed at the extremity; a part of which, together with beds, slops, marine clothing, &c., (at present stowed in the after cock-pit) I proposed should occupy the present place of the sea-stores in section No. 5; which would reduce the excess of weight at present in the after extremity, and place less weight, more lively, and more speedily consumed than the sea-stores, in the place now occupied by them.

Such arrangements would also be likely to contribute to a more equal distribution of the weights and pressure in sections 3 and 4; as it would most probably admit of weight being removed from the one where there is at present an excess, to that where pressure preponderates,—and, in fact, to a much more equal distribution throughout the whole body.

The following alterations would also contribute to a better distribution than the present.

Let the bower and sheet cables be stowed further aft; the stream cable and hawsers abaft them, in the space now occupied by cabins, slop-room, &c.; and the stores, at present stowed in the foremost section No. 5, removed aft to the fore part of the cable tiers; a part of the bread, slops, beds, marine clothing, &c., as in the former proposal, to occupy the present place of the sea-stores in section No. 5. By such arrangements the weight of the stream cable and hawsers, and part of the bower cables, would be removed to section No. 2; the sea-stores to No. 4; and the weights reduced at both extremities.

The excess of weight would not only be considerably reduced in the fore extremity, by the proposed alterations; but at sea, if desirable to lighten this part as speedily as possible, a regular decrease, in a 74-gun ship, of two tons per week, may be calculated on, till the expiration of half the cruise, when the fore bread-room would be cleared.

By a minute inspection and measurement of the different parts, and a correct calculation of the different weights and their bulk, the distribution may be made with great exactness.

Removing the anchors where they will stow perfectly clear of the ports, and similar to the sheet and spare anchors, will also leave the round of the bow perfectly clear, and remove every impediment to its being better fortified.

Plymouth, Feb. 14*th*, 1829.

Geological Notices of St. Helena suggested during a Visit to that Island in 1828, *in a Voyage from India to England.*

By Charles H. Weston, Esq.

The island of Saint Helena is interesting to the visitor on every account. Whether he may view it as the little spot which has formed the last and circumscribed retreat of one who in the days of his glory and ambition thought the wide world scarce sufficiently extensive; or whether he consider its peculiar scenery abounding in contrasts—of luxuriance and sterility, between whose rugged and naked hill-tops and grass-green vallies there is no such gradation as the English eye is accustomed to dwell upon; or whether he look upon its volcanic characters, and reflect upon the period when nature here exhibited such awful proofs of her irresistible power,—it will be found equally capable of exciting his interest. But it is in its last character that we are interested, and about which I would offer some remarks. These remarks, however, must unavoidably be few in number, and somewhat superficial in their nature, from the limited residence which the shipment of necessary supplies usually affords to passengers in their voyage from India to England.

But while stating the characters of the few geological specimens I collected and procured, I shall not hesitate to make use of the information of others, whose opportunities have afforded them better local knowledge; and if any hint should lead other and more able travellers to direct their attention to the geology of this singular island, so as eventually to throw any light upon the interesting problem,—"Whether St. Helena is truly a volcanic product, or the remnant of a much larger portion of land now submerged," *—an object of no small importance will have been attained.

In approaching St. Helena from the southward, we sail round the island from the south-east to the north-west, so that a great line of coast is presented to our view, and as the shores are so precipitous as to allow a ship to sail within almost two cables' length of the land, the whole coast is distinctly seen and may be easily examined. But I did not in any part recognise rocks belonging to the primitive formation; neither did I meet with any during my rides in the interior. From General Walker's geological notices * also, it is evident that he had never met with any of this class; and as Governor Beatson is equally silent on this subject, we may conclude that primitive rocks do not exist in the island.

---

* Vide Beatson's "Tracts relative to St. Helena.; Daubeny on Volcanoes. Philosophical Magazine, No. 342.

The perpendicular coast seems to give strong evidence of the convulsions which St. Helena must have experienced at some remote period; for not only is it intersected with deep ravines, but at the northern part there appeared to be nearly a complete separation of the cliff from the top to the bottom. The whole stratification also is much disturbed, and dips in every direction; at one spot I recollect seeing the strata of a hill to dip on either side to the contiguous vallies.

There are many parts which are beautifully intersected by red veins, composed of alumina, highly impregnated with the red oxide of iron.

In speaking of the general characters of St. Helena, I would only remark, that the volcanic matter chiefly consists of a rock of a basaltic nature†; and therefore, when we read, that "obsidian or pumice has never, I believe, been found here," ‡ we see a reason why the latter substance, at least, should not have been found, as it is the product of trachyte or feldspathic lava, and not of augitic or basaltic lava §.

But before describing the interior of the island, I will here subjoin a list of some geological specimens, with their localities:—

No. 1.	Basaltic lava, compact, .	Ladder Hill.
2.	Ferruginous schistos, . . .	Ladder Hill.

---

* Vide Philosophical Magazine, No. 342.

† See also Daubeny. ‡ See Philosophical Magazine, No. 342.

§ See Daubeny, p. 24, et passim. Analysis of different substances in Ure's Chemical Dictionary; and article "Pumice," Rees's Cyclopædia.

No. 3.	Ancient lava (ferruginous)	Ladder Hill.
4.	Ancient lava (ferruginous) one side blackened and vitrified, with part converted into sponge-form lava,	Rupert's Hill.
5.	Ancient lava, highly impregnated with the red oxide of iron,	Ladder Hill.
6.	Do. do. do. passes from the close texture of Nos. 3 and 4, to the cellular of No. 7,	
7.	Cellular lava, in grain similar to No. 1,	Ladder Hill.
8.	Scoriaceous lava,	
9.	Spumous lava,	
10.	Semivitrified clay,	
11.	Brown Jasper (common massive) *,§	Break-neck Valley.
12.	Pitch stone, approaching red jasper §	Lot's Wife.
13.	Semi-opal (amorphous) §	Gregory's.
14.	Indurated lithomarge,	Unknown.
15.	Lithomarge converted into a kind of porcelain,	
16.	Stalactite, §	Sandy Bay.
17.	Do. massive, zoned, and agate-like, §	
18.	Globular aggregated octohedral crystals of sulphate of lime, §	Sugar Loaf.

On reading over the above enumeration of specimens, we are immediately struck with this very singular fact:—that St. Helena must have suffered at least *three distinct volcanic eruptions*. Nos. 1, 3, 4, 5 and 6, 7 and 8, are the specimens here referred to. These lavas were evidently formed not only at three distinct periods, but, as their mechanical texture clearly shews, under totally different circumstances. The effects of heat, in the formation of each lava, have been modified by the influence of pressure. †

The incalculable weight of the ocean superimposed, would effectually prevent the escape of gaseous products, and thereby give to the fluid rocks that dense character and that freedom from porosity, which basaltic lava of this kind (No. 1.) always presents. Lava such as this could not have been formed without pressure; I would, therefore, denominate it—"*submarine lava.*"

---

* The situations of those marked §, are given on the credit of the person from whom they were bought: the rest I collected on the spot.

† See the different effects of pressure ably explained by Daubeny.

The lavas of numbers 3 and 4 appear to be intermediate between numbers 1 and 7 and 8. The grain is close but penetrated with minute pores; evidently formed under less pressure, which was not quite sufficient to prevent the escape of gaseous products.

It might at first seem that the intermediate lavas had been formed after a partial subsidence of the waters of the deluge, (to which we shall soon again refer); but the specimen marked No. 6, possessing in itself both porous and cellular lava, proves it to have suffered a diminution of pressure, even during its ejection. We are, therefore, rather led to attribute this effect to the more sudden rise of the volcanic island, than to the result of the more gradual removal of pressure, which the first hypothesis would involve.

This lava, also, was therefore formed *entirely under water*, although, when compared with the first eruption, at no great depth.

If we thus suppose the two first lavas to have been formed previous to the total subsidence of the waters, the great argument which Beatson derives from the level plains of "Long Wood," "Dead Wood," and others, against the volcanic origin of this island, will lose much of its force. Under these supposed circumstances, this tertiary formation of "soil and clay" must have been regularly deposited over this yet submarine land, with its surface necessarily smooth and "level." The existence also of "considerable beds of small stones resembling gravel," of which Governor Beatson makes mention, is a collateral proof of the agency of the sea, in the formation of these "Great Plains." *

I shall, for the sake of distinction, term these first lavas *antediluvian;* because ejected at least previous to the total subsidence of the waters.

But not till after this event (the total subsidence of the waters) must the eruption of the lavas numbered 7 and 8 be supposed to have taken place; and that this third eruption occurred since the ejection of the second and intermediate lava seems evident from the specimen No. 4, which has one side blackened and vitrified, and converted into sponge-form lava; some fire at least, therefore, must certainly have acted upon it subsequent to its eruption. But this supposition seems confirmed and placed beyond a doubt by the great cellularity of these lavas, which as it is the natural result of the free and unrestrained extrication of the various gases during the fluidity of the rocks, proves their ejection to have happened under no greater pressure than that of the atmosphere; and, therefore, the third class of lavas might be termed, in contradistinction to the two kinds before referred to, *aërial lava.*

---

* "This soil a little north of the 'Great Wood,' is strongly impregnated with salt; nothing grows upon it but marine plants."—*Beatson.*

The ejection, however, of this last highly cellular and scoriaceous lava is no less general; it was confined to *particular parts* of St. Helena, in some places destroying the former character of the island, after the Neptunian deposit; and in others leaving it untouched and unaltered. *Hence* the formation of the " Great Plains," and *hence* would arise that " striking dissimilarity between the exterior and interior parts," from which principally " others contend that it is the remnant of a large island, the greater part of which has been sunk under water, by the force of earthquakes or volcanoes." *

With such views, then, of the subject, there does not seem to be such cogent reason for arriving at the conclusion, that,— " upon the whole there appears to be strong grounds for supposing that the finest parts of the interior are the remnants of primitive land." †

But I would also remark, that the face of this island has been made to present still more " striking dissimilarities" in its different parts, owing to the convulsions which have taken place even since the last lava burst forth.

St. James's Valley itself has been the result of one of these convulsions. The lava appears to have flowed " from a crater somewhere about the site of the Water-fall,"* in a north-westerly direction, forming a high hill as far as the sea-shore. At some subsequent period a tremendous explosion has rent the hill from the top to its base, forming Ladder Hill, which lies west of the town, and Rupert's Hill, which lies to the east. This is most plainly and satisfactorily evidenced by the relative situations of the hills, and the exact correspondence and inclination of the strata.

---

* See Beatson's Introductory chapter.

† Beatson, p. 9.

I was happy to find, on perusing Governor Beatson's "Tracts," that his opinion, founded on a more extensive and careful survey of this spot, perfectly coincided with my own.

This, perhaps, is not the only instance of these (geologically speaking) modern earthquakes.

Mr. A. Blaxam of H. M. S. Blonde, in describing another part of this island, writes thus: † "The ridge termed Diana's Peak forms one edge of the crater: it is entirely composed of lava, the greater part of which is in a high state of decomposition. The ridge as it approaches the sea inclines towards it; so that we may suppose the remaining ridge, which is wanting to form the edge of a complete crater, lies buried in the sea."

If this crater (as from this account seems the probable conclusion) was formed by the eruption of lava, the burying of one side in the sea must have occurred subsequent to the event.

Another source of scepticism, respecting the entire formation of this island by volcanic means, appears to arise from the consideration of the unbroken ridge of hills which runs east and west.

"In no part of this ridge," remarks Governor Beatson, "(which is elevated 2,000 feet above the level of the sea,) is there a single chasm or opening. It seems therefore wholly unaccountable that it should have escaped being broken or shattered, if earthquakes or subterranean fires had occasioned the overturnings."

So far, however, from considering this unbroken elevation as militating against the theory before advanced, I think it may be adduced as a corroborative proof in its favour. I will endeavour to explain.

---

* Beatson, p. 2.

† See Philosophical Magazine, No. 342.

The singular manner in which all the vallies converge from the circumference of the island to the Peak of Diana, as to a common centre, * seems to point out this spot as the part upon which the disruptive force of the elastic fluids from below at first chiefly and more directly acted.

The insulated hill before referred to is, indeed, on a small scale, a good exemplification of this kind of formation: for the direction of the strata, dipping "on either side to the contiguous vallies,"† could only have been given by the expansive force of elastic vapours acting upon, and heaving up, a portion of the earth. The peculiarity of the vallies of this island, it may be also remarked, seems somewhat analogous to the effects which Von Buch describes as always resulting from elevations of land, unaccompanied by any ejection of lava. ‡

But while the centre of Saint Helena was chiefly acted upon, the *grand course* of the subterranean fires seems clearly to have been *east and west*, because the submarine base of this island (upon which by-and-bye I shall make some observations) extends at least twenty-five miles in this same direction. The soundings also which were made under the orders of Captains Cowan and Beville shew us, that the ground westward consists of "hollow ridges, apparently resembling the surface of the island." § The natural consequence of such an *extended* line of subterranean agency lifting up the earth would be to form a lengthened elevation, which, if in any degree gradually and equally raised, would present one unbroken ridge, "without chasm or opening."

The extended base of the island seems to have become, with some, another argument against its volcanic origin.

Saint Helena "is not a rock rising abruptly" out of the water, but extends under the sea, "at least twenty-five miles from east to west." §

---

* See Beatson's Geological Plan and Elevation of St. Helena.

† Vide *supra*.

‡ Although the last lava appears to have flowed from many parts, it was not thrown out *en masse* from its centre.

§ Beatson, p. 12.

But while this circumstance proves to us, that the volcanic action below was much more general than the bare inspection of the emerged part of St. Helena would, perhaps, have allowed us to have calculated; it most forcibly reminds us of the great similarity which exists between this portion of the world and others where volcanic eruptions have taken place—I mean the very extended action of subterranean fires. Etna, with its Lipari, Vulcano, and Stromboli; Hecla with its submarine eruptions at eight miles distance, and at even seventy; and the isles of France and of Bourbon, immediately present themselves as good illustrations of this fact.

That any one should be influenced in his decision respecting the volcanic origin of St. Helena by the supposed former existence of the "Atlantis" is somewhat singular. Geological arguments, and not literary, are alone to be made use of in the question now under consideration; and literary shall I indeed esteem them until the real existence and true geographical situation of this great island of Plato be clearly demonstrated and indisputably pointed out,—whether it may be in Sweden and Norway, according to Rudbeck; in the frozen ocean, according to M. Bailli, or in the Utopian land of fable, according to the more reasonable opinion of Sir William Jones.

But it is fit that these few, and perhaps too general, remarks should be brought to a close, and I would do so without asserting dogmatically the necessary tendency of the evidence resulting from them; for whether this hypothesis, respecting the formation of St. Helena, carries with it any plausibility, or the observations which a perusal of the ideas of others have elicited possess any value, I leave entirely to the reader's decision. I offer them diffidently, desirous rather of leading others to make this singular island an object of philosophic investigation, than anxious to make converts to any peculiar views I may be thought to have entertained upon the subject.

Refutation of the Charges contained in Mr. Ivory's "*Remarks,*" *&c. in the Philosophical Magazine and Annals for February last.*

From looking into Mr. Ivory's remarks, I find they have assumed that form which all his controversial discussions do when he has the wrong side of the question; which, in the present instance, he has in a superlative degree—for the doctrines which he compels himself to maintain are of the most untenable and contradictory description: but no matter for that, he must still keep up the farce and repeat the old chorus. In such circumstances, as is well known, Mr. Ivory labours hard, by a variety of ways, to obscure the subject, to divert the reader's attention from the real merits of the question, and to excite the indignation of the public against his opponent, by alleging—no matter on how ridiculous, frivolous, or unfounded grounds—that some advantage has been taken of himself—that some fraud has been committed; such as, in my case, "giving to Mr. Ivory's formula any shape I please,"—using the black art of "legerdemain,"—"playing tricks with the algebraic expressions," &c.

Charges like these, when true, call for exemplary visitation, especially playing off legerdemain with such a grave gentleman as Mr. Ivory; but as I am not aware of having practised any thing of the sort, and as no intelligible evidence is offered of my having played any tricks, I neither see cause for apology, nor need for reform. On the contrary, it is easy to show the futility of all these accusations, and that they may, with far greater justice, be charged upon their author. Indeed, in this case there is no need for playing tricks, or resorting to any unfair means whatever; for let Mr. Ivory take up any position he pleases, provided he retain, as he still obstinately does, his favourite but incompatible assumptions, and it is easy to prove the extravagant inconsistency of his doctrines, whether among themselves or with the best known facts.

The first charge preferred against me—a serious and criminal one too, had there been the shadow of truth in it—is, that "I am bent on giving to *what I call Mr. Ivory's formula* any shape, and on making any use of it I please," &c.

As a complete refutation of this grave accusation, I need only observe, that throughout the whole of my reply I have never once mentioned any such formula of Mr. Ivory's, far less turned it into my own shapes: being well aware how different *his* formulas are from his new law of condensation, which, and not *his* formulas, was the thing I then wished to discuss. For this reason I kept clear of Mr. Ivory's ever-changing formulas altogether, and employed a formula of my own framing, which, as I shall presently show, fairly and faithfully represents his new law of condensation, while *his own formula* does not. His celebrated new law is, that "*the heat extricated from air when it undergoes a given condensation is equal to* $\frac{3}{8}$ *of the diminution of temperature required to produce the same condensation, the pressure being constant.*"

To obtain a formula which shall fairly express this law, let V' be the initial volume of a mass of air, and V any other volume to which it is to be reduced, while ϱ' and ϱ are the corresponding densities. Then, as is well known, the volume of a mass of air under a constant pressure varies as its temperature reckoned from $-448°$ *F.*, or as $448° + t$; where t is the degree on Fahrenheit's scale. The volume would therefore be diminished from V' to V, or the density increased from ϱ' to ϱ, under a constant pressure, if we diminished its temperature reckoned from $-448°$ *F.* in the ratio of V' to V, or made it equal to

$$\frac{V}{V'}\left(448° + t\right);$$

so that the diminution of temperature required to produce the given condensation is

$$448° + t - \frac{V}{V'}\left(448° + t\right);$$

three-eighths of which $\left(\text{because } \frac{\varrho'}{\varrho} = \frac{V}{V'}\right)$ is

$$\tfrac{3}{8}\left(448^\circ + t\right)\left(1 - \frac{V}{V'}\right) = \tfrac{3}{8}\left(448^\circ + t\right) \times \frac{\varrho - \varrho'}{\varrho}\,;$$

the very formula which I formerly employed, without ever giving Mr. Ivory the credit of framing it.

We have thus again arrived at that notable formula expressing Mr. Ivory's new law of condensation, from which there emanates such a catalogue of unparalleled results. Among these, as I formerly shewed, we have tinder kindling at a temperature below that of boiling water—a cold below *absolute* cold—a greater heat caused by twice doubling the density of a small quantity of air, than if all the air in the universe were instantly condensed into a point!

It remains to be shewn that Mr. Ivory's *own* and ever-varying formulas do not accord with his new law. Towards the bottom of page 105 he gives the following formulas, for the change of temperature due to changing to volume from V' to V, or the density from ϱ' to ϱ; namely,

$$\frac{1}{\beta}\left(1 + \alpha\,\theta\right)\left(\frac{V}{V'} - 1\right) = \frac{1}{\beta}\left(1 + \alpha\,\theta\right)\left(\frac{\varrho'}{\varrho} - 1\right)$$

where θ is the Fahrenheit temperature $\frac{1}{\alpha} = 480^\circ$ and $\frac{1}{\beta} = 180^\circ$;

whence the formulas become

$$\tfrac{3}{8}\left(480^\circ + \theta\right)\left(\frac{V}{V'} - 1\right) = \tfrac{3}{8}\left(480^\circ + \theta\right)\left(\frac{\varrho'}{\varrho} - 1\right).$$

Now when Mr. Ivory announced his new law, *Phil. Mag.* for Feb. 1827, page 94, he illustrated it by an example; where the volume is to be halved and the initial temperature is 32° *F.*; whence he declares the rise of temperature due to halving the volume to be 90°. But if in the last formulas we make V half of V' or ϱ double of ϱ' and $\theta = 32^\circ$, we do not obtain 90° but 96°. A clear proof that Mr. Ivory's *own* formulas are very different from his new law, though they involve as great absurdities.

This strange discordance seems to owe its origin to the following circumstance. For the sake of obscurity, and lest any one should easily read through his untenable doctrines, Mr. Ivory is perpetually shuffling his formulas, and substituting one for another in endless succession. He is, moreover, continually shifting between Fahrenheit and the centigrade scale; so that he never writes two papers in succession where he uses the same scale of temperature. It is of no moment which of the two scales be employed; but Mr. Ivory knows well that it is a sad embarrassment to the reader to shuffle between them. Now it would appear, that, in thus zealously endeavouring to make the reader lose sight of the subject, Mr. Ivory has deservedly fallen into his own snare, so as to commit the blunder above mentioned.

I am called upon, it seems, to go back to the principles from which Mr. Ivory's formula is deduced, and to his usual theory of the thermometer. On this head I need only refer him to my other paper, which, I presume, will appear along with this. In it he will have an additional opportunity of seeing an exposure of the utter inconsistency of these principles, and therefore any deduction from them must be of very little worth, however anxious he may be to keep up the farce a little longer. If a formula be in itself absurd, it little matters from what it be deduced, for that cannot legitimize it. But one thing is now evident, and that is the grand point at present, that, without calling it Mr. Ivory's formula, I had used a formula which faithfully represents his new law of condensation, with all its concomitant absurdities.

Where, then, is the shadow of evidence of my having, in the least degree, changed the shape of Mr. Ivory's formula—of legerdemain—playing tricks, and such like ridiculous allegations? Is it not highly creditable to Mr. Ivory's talents and veracity to be able to bring forward all these accusations without accompanying them with one syllable of proof? He has announced that he will not again return to the subject. There are few, I dare say, but will admit that it is high time he should retire, unless he have something more substantial than mere *ipse dixit* to bring forward. This dispute must surely have lain very near Mr. Ivory's heart, when, for want of every admissible argument, he has been laid under the necessity of advancing so many unsupported, nay unfounded, statements, by way of defence. There is one consideration, however, which even in retiring must afford him no small consolation, namely, that he had the honour of beginning this controversy himself.

In conclusion, I only beg to recall the attention of Mr. Ivory and others, who dote upon the analytical theory of sound, to an objection I formerly stated, and which, so far as I know, had not been before attended to; namely, that in all these fluxionary investigations, each small cylinder of air is supposed to vibrate so as to have its variable length exactly proportional to its velocity in every part of a vibration. Consequently, when the velocity becomes *nothing*, at the turn of each vibration, the length of the cylinder is also *nothing;* that is, the cylinder is *annihilated* at the turn of each vibration. Now, I would ask these philosophers, if this circumstance alone would not be fatal to the fluxionary investigations, although all other objections were laid aside?

HENRY MEIKLE.

A New System of Geology, in which the great Revolutions of the Earth, and Animated Nature, are reconciled at once to Modern Science and Sacred History. By ANDREW URE, M.D., F.R.S., &c. London: Longman, Rees, Orme, Brown, and Green. 8vo. pp. 680; with seven plates and fifty-one wood-cuts. 1829.

WE gladly embrace the earliest opportunity of presenting our readers with an analysis of this interesting, and in many respects original, work. The author has been long esteemed among men of science for his able and intrepid refutation of numerous errors current in some of our chemical systems; and he has now directed, with a no less steady aim, the same vigilance of observation and logical acumen towards the mystifications of the mineral world. Though the book now before us has not in the least a controversial texture, yet its author could not possibly avoid noticing, in his introductory dissertation, the chimæras of cosmogony which at no distant date fluttered in many a geognostic head; and there is something truly picturesque in the athletic *nonchalance* with which he strangles them at a grasp, instead of nibbling at their wings and claws, as the rival partisans of those reveries were wont to do.

But Dr. Ure's present labours are not all of the *Augean* kind. Not content with removing much of the rubbish which has been heaped around rational geology, he has apparently got a glimpse of the true architecture of the globe, and has delineated some of its outlines with a powerful hand.

The great mystery of the mineral kingdom is to be found in the remains of the tropical plants and animals so profusely buried in the secondary and diluvial strata, not only of our temperate countries, but of even the circumpolar zones. Their delicacy of fabric, integrity of form, and posture in the earth, repel the idea of their having been transported to their present sites from the distant equatorial regions, by any aqueous catastrophe. They are undoubtedly buried in the spot where they grew and perished. It is to this enigma of the high temperature which anciently prevailed in our parallels of latitude, that Dr. Ure has concentered all his faculties of research and illustration, and to certain sudden *crises* of refrigeration which seem to have supervened at distant intervals,—the last of them being the Noachian Deluge. That prior to this event, the terraqueous equilibrium was unstable, many great geological phenomena clearly attest ; and the Doctor, in discriminating them, endeavours to shew whence that instability proceeded.

These propositions, as now picked out by us in an insulated form, may have somewhat of a hypothetical aspect, but no such impression will be felt in studying the work itself, since they spontaneously result from the phalanx of facts which he has embodied in their service.

There is one thing which we like exceedingly in the Doctor's schemes of induction ; the documents subservient to them are neither got up nor trimmed by himself, but are taken, without distinction, from every record of genuine observation, whatever may have been the theoretical creeds of their authors.

It is amusing enough in this tesselated scheme of induction, to see conclusions legitimately drawn from facts which were originally advanced in support of entirely different views. But such is the influence of our author's logical collocations, that we can rarely help assenting to them, as highly probable, at least, if not absolutely true ; and this is nearly as much as can be said for all physical doctrines not directly drawn by geometry from observation or experiment.

It strikes us that the title of this work might have very properly been, *Geological Physics*, or the *Philosophy of Geology*: for not more than one-half of it is occupied with geological details, and even these are subordinate to general inductions.

The introduction is in reality an elaborate philosophical discourse, in which several important preliminary questions are discussed: such as the visionary spirit of cosmogonists, ancient and modern; the connexion of geology with Scripture history, and of science with revelation in general; the illogical procedure of geognostic theorists, exemplified particularly in the Huttonian and Wernerian systems, as expounded by Playfair and D'Aubuisson; the new era of practical geology, commencing with Mr. Smith's sections of English strata, and the foundation of the Geological Society of London; the importance and uses of organic remains, &c.

One of the author's motives for publishing the present work will receive the unqualified approbation of all good members of society,—" a wish to lead popular students of philosophy to the moral and religious uses of their knowledge;" and, consequently, to expose the fallacies of our modern Pyrrhonists who ransack nature apparently with no other end than to call in question or vilify the wisdom and goodness of the Creator. Under this head, we find the limits of final causes well defined, and their temperate application ably vindicated.

Among the champions who have figured in the arena of cosmogony few have been more extolled than Professor Playfair, the eloquent historian of physical science; yet in our humble apprehension, few have so frequently violated every rule of induction as he has done in his geological speculations. We feel, therefore, truly grateful to Dr. Ure for the moral courage with which he has dispelled this illusion of a great name, so injurious to the advancement of truth.

" The theory of Hutton was passing fast into oblivion, like its visionary predecessors*, when it was re-embodied, for a season, by the eloquence of Playfair. Delighted with the imposing boldness of the Huttonian creed, the Professor undertook its exposition with the ardour of a mineralogical neophyte. Bringing to the task the joint resources of dialectics and geometry, he produced those *Illustrations of the Huttonian Theory* which have been so highly celebrated by his literary friends, though they will probably add nothing to his fame as a philosopher."

---

* The cosmologies of Leibnitz, Whiston, Demaillet, Buffon, Dolomieu, Bertrand, &c.

After a few introductory remarks, Dr. Ure quotes a passage from the illustrations, on which he comments in the following terms:—

"Had the Professor regarded the hypothesis of his friend, Dr. Hutton, with equal impartiality as that of Werner, he would not have expended any refined analysis in its support. That the planets had at one time 'an actual figure *infinitely* more irregular' than the present, is certainly a very strange proposition for a modern philosopher to make, one equally extravagant with any Epicurean fiction in Lucretius. After an indefinite period, it appears that one of them at least, the earth, became sufficiently regular for an abode of living beings. Their habitation has, however, a precarious existence. The progress of waste will eventually crumble down its mountains, and strew their *detritus* over the bottom of the sea, converting our imperfect spheroid into the perfect form of equilibrium. This geometrical symmetry which conspiring physical forces tend, by this creed, to produce, should some day cover the whole land with a winding-sheet of water, universally fatal to organic motion and life. This mortal consummation, however, they propose to obstruct from time to time, by fortuitous explosions from the central fires, upheaving mountains and plains, as they are wanted, to rescue the world from a watery grave. The probability of such a mode of salvation is rapidly diminishing; for it is admitted by the theoretic votaries both of Vulcan and Neptune, that these igneous eruptions are becoming vastly feebler and less frequent than they were in ancient times; that volcanic fires are fast expiring, and only a few smoking spiracles remain to attest their former activity. In this predicament, the Huttonians can hold forth to proselytes but slender hopes of the duration of their system. The casual convulsion of a dying power is a very precarious resource, and can be little relied on for resisting the steady pace of destruction. The earth of the Huttonians must become a finished spheroid, unfit for every useful purpose.

"Mr. Playfair's two travellers, the celestial and terrestrial, would have found a better coincidence, and one more to their credit, as philosophers, by tracing at once the actual and only beneficial form of the earth, and its fellow planets, to that perfect Wisdom which created all things in conformity to their destined ends, and which provides, with unwearied beneficence, for the wants and well-being of every organic tribe.

"I have no desire to fatigue my readers with a detailed examination of the theory propounded by Hutton, and embellished by Playfair. Its defects and inconsistencies, and indeed its whole hypothetical tenor are now so notorious, that no practical naturalist of eminence will venture to adopt its conclusions. My sole object, here, was to unveil its vain spirit of theoretic cosmogony; to exhibit its efforts to build a Babel-tower that should make a name, and enfranchise it from the control of a creating and directing Providence. The world, according to Hutton, shows no trace of a beginning, or of an end; but has been the theatre of an indefinite series of transformations in time past, and will continue to be so in time to come. The mountains of a former earth were worn down and diffused over the bottom of a former ocean. There they were exposed to the agglutinating power of subjacent internal fires; and after due induration, were heaved up by the explosive violence of the same force, into the inclined or nearly vertical positions, in which the great mountain strata now stand.

"The mountains of gneiss and mica slate, allowed by every practical geologist to be primitive, are barren in animal exuviæ. Now these most distinctly stratified rocks were formed, it is said, at the bottom of the Huttonian sea, by the same process as the calcareous and other secondary strata that are replete with shells. Whence do these organic ruins come, and why are they absent in the former class of rocks, both of them formed in the same sea, and under similar circumstances? They cannot reply that the epoch of the gneiss and mica-slate formations was anterior to the existence of animals; for their theory affirms, that the present earth sprung up out of a preceding one, by a spontaneous growth or transition, without the intervention of a divine creative energy. They tell us, that in an indefinite series of ages, the mountain masses of the pre-existing globe became submarine concentric layers of rock, which were thence elevated by catastrophe into the present dry lands. Their marine deposition was slow and tranquil, disturbing the general economy of nature no more than at present, and, consequently, not interfering with the production of marine testaceous animals, nor with the distribution of their shelly exuviæ. Hence, should these beds be eventually indurated and heaved up by the subjacent fires into the nearly vertical mountain schists of a new earth, they must contain the organic witnesses of their submarine formation. But since our actual mountains of gneiss and mica-slate are destitute of these internal witnesses, as also of their basis, carbonate of lime, they cannot have been formed at the bottom of an ocean teeming with animal life. Devoid of organic remains, they indicate a sea devoid of vital energy. The first appearance of shelly strata is coincident with a specific exertion of creative power, "This mortal chasm, in the succession of mineral formations, breaks the chain of the Huttonians. It is the death blow of their theory; demonstrating that the present earth has resulted from definite creative *Fiats;* and not from the progressive operations of any merely physical forces whatsoever. It is therefore to be regretted, that a mind so accomplished as that of Professor Playfair should have devoted so many studious years to the decoration of the phantoms described by him in the following paragraph.

"'How often these vicissitudes of decay and renovation have been repeated, is not for us to determine; they constitute a series, of which, as the author of this theory has remarked, we neither see the beginning nor the end; a circumstance that accords well with what is known concerning other parts of the economy of the world. In the continuation of the different species of animals and vegetables that inhabit the earth, we discern neither a beginning nor an end; and in the planetary motions, where geometry has carried the eye so far both into the future and past, we discover no mark either of the commencement or of the termination of the present order. It is unreasonable, indeed, to suppose that such marks should any where exist. The Author of Nature has not given laws to the universe, which, like the institutions of men, carry in themselves the elements of their own destruction. He has not permitted in his works any symptom of infancy or old age, or any sign by which we may estimate either their future or past duration. These phenomena, then, are all so many marks of the lapse of time, among which the principles of geology enable us to distinguish a certain order, so that we know some of them to be more, and others to be less distant, but without being able to ascertain, with any exactness, the proportion of the immense intervals which separate them. These intervals admit of no comparison with the astronomical measures of time; they cannot be expressed by the revolutions of the sun or the moon; *nor is there any synchronism between the most recent epochas of the mineral kingdom, and the most ancient of our ordinary chronology.*'

"Our ordinary chronology comprehends the deluge, a great epocha of the mineral kingdom, the truth of which is obviously discarded by Mr. Playfair. In the third part of the present work, ample evidence will be adduced from Cuvier and other practical naturalists, of the reality of that recent epocha, and of its synchronism with our chronology. Moreover, it will be shown in our second part, that the mineral strata contain formations, which 'discover marks of the commencement' of the different species of vegetables and animals that peopled the earth. The astronomical comparison is a strange solecism for so acute a logician to commit. The cases are quite discrete, and destitute of any true analogy. The laws of the planetary motions are represented in a system of mechanical theorems, which relate solely to *co-existing* phenomena. The principles of their actual equilibrium apply equally to all past and future time. The appearances reveal nothing in the past, or the future, different from the present, except change of relative position among separate masses. The physical constitution of the planets, which could alone afford, in their anterior metamorphoses, kindred or parallel facts for geology, are beyond our cognisance. In the mineral structure of the earth, we shall find symptoms of infancy, as well as on its surface, considered as the dwelling-place of man."

Our author gives the Wernerian theory fair play, in judging it by D'Aubuisson's temperate representation. On this, its most reasonable form, we find the following observations; in which we perfectly concur.

" It would be superfluous, indeed, to offer an elaborate refutation of a world-building hypothesis, so extravagant, so visionary, and so inconsistent with every principal of mechanical and chemical science. Whence arose that immense chaotic ocean, within whose bosom the summits of the Himmalaya and the Andes were crystallised? Whither did it retire in measured stages of descent, to allow the primary and secondary rock formations, to lay their successive platforms, in the amphitheatre of the globe? The atmosphere has a finite extent, not expanding into space beyond a limited distance from the earth; and thus that world of waters could not escape into another sphere, in vaporous exhalations. The quantity of water requisite to cover the globe to the height of the Himmalaya, or 27,000 feet, would be as great as the whole mass of our actual ocean. Werner was too little of a philosopher to calculate that his crystallisation plan called on him to provide a receptacle for 1000 millions of cubic miles of water. The great density of the interior body of the earth precludes the possibility of that receptacle being subterranean. Since neither celestial nor terrestrial space will admit his retiring chaotic ocean, itself must be deemed an inadmissible supposition. Even granting its reality, and allowing moreover that this water was adequate to dissolve the now insoluble granitic mountains, so as to form a clear and tranquil fluid, we may ask, why the solvent that then exercised so marvellous an affinity for the siliceous and aluminous earths, came so soon to discard them altogether from its bosom? The attractive force that made the solution should have maintained it. To imagine an effect to come to pass without a cause, is sufficiently ridiculous, though not without a parallel in modern philosophy; but to expect a solidifying action, a stony deposit, from a liquefying agent, unabated in force and magnitude, is truly absurd. The other assumption of the clear and tranquil solution becoming spontaneously muddy and disturbed, as it parted with its solid contents, is repugnant no less to common sense, than to chemical experience. A still liquid containing soluble and insoluble matter usually deposits the insoluble, which is mechanically diffused, before the crystals appear, provided the latter be regularly aggregated, as occurs in granite. We shall not waste more words on this analysis; but conclude with expressing our astonishment that the theoretic dreams of Werner should ever have been regarded as realities, in the present age."

The body of Dr. Ure's work is distributed into three books; each containing several chapters. The first book comprehends what may be called, on Dr. Ure's principles, *primordial creation*, including primitive rocks. Chapters I. II. III. treat respectively of heat, light, and the atmosphere. In Chapter IV., entitled "Primeval Land and Ocean," some of our author's new thoughts on the structure and revolutions of the earth begin to appear.

"Circumstances, which will transpire in the progress of our researches, cause me to assign a different proportion between the dry and humid surface in the primeval globe, from that which now prevails. I am led to conclude, that the area of the land bore to that of the water, probably the ratio of equality, instead of nearly 100 to 365, as at present. One obvious consequence of this condition was, that the depth of the primeval ocean was greater, as its superficial expanse was less. By this means, the water came into far deeper and more extensive contact with those ignited, combustible, and explosive materials, which the phenomena of mines and volcanoes demonstrate to exist within the crust of the globe."

. . . . .

"Multiplied observations have shown, that the crust of the earth is composed superficially, or to a moderate depth, of certain stratiform or schistose rocks, which, being devoid of organic remains, are termed primitive. We shall at present confine our attention to two of them, called gneiss and mica-slate. These are arranged in planes usually parallel to each other, the mica-slate being, for the most part, uppermost. We have reason to believe, that hardly any district of the terrestrial surface is destitute of these great slaty rocks, though in many places they may be deeply covered over with secondary formations, and therefore inaccessible. Gneiss constitutes the body of the Himmalaya mountains, and abounds among the Andes, Alps, Urals, Pyrenees. It forms also Ross island, the most northern known land of the globe. Mica-slate is nearly co-extensive. But their wide-stretched foliated planes are seldom or never horizontal, or concentrate with the curvature of the earth. They usually lie at highly inclined angles, like tables resting on their edges, in a nearly vertical position. In very many localities, vast irregular masses of granite are seen rising up through the schistose fields, as if these had been upheaved and dislocated by its protrusion, and were thrown like mantles round its shoulders and base.

" We therefore conclude, that the primordial earth, as it lay beneath the circumfused abyss, was at first endowed with concentric coats of gneiss, mica-slate, and the other primitive schists; that at the recorded command of the Almighty, a general eruption and protrusion of the granitic, syenitic, porphyritic, and other unstratified rocks, took place, which broke up and elevated the schists into nearly vertical planes, similar to what now exist, leaving commensurate excavations for the basin of the sea.

" In meditating on this mighty operation, though we may shrink from the overwhelming scene, and feel our faculties abased, nay annihilated as it were, in the presence of that Power, ' who hath measured the waters in the hollow of his hand, weighed the mountains in scales, and the hills in a balance; who sitteth upon the circle of the earth, and stretcheth out the heavens as a curtain,' yet the magnitude of these terrestrial disruptions will create no difficulty in the mind of the astronomer, familiar with acts of Omnipotence incomparably more stupendous. Even the geographer would smile at the geologist, who should ask for Deity a countless lapse of ages to build upon the earth its superficial scaffolding, whose size is to its total bulk, as the roughness of the rind to the ball of the orange."

The constitution of water, as described in this chapter, shows what interest a philosophical spirit may infuse into the tritest subjects.

" This water, by its mysterious tenuity, loosens the indurated soil, enters the invisible pores of plants, passes freely through all their vessels, expands in the filmy blossom, and is an element of the fleeting aroma. But these fluid particles can be chained together in the firmest cohesion: in which state it may exhibit either the hardness of rock, or the softness of eider-down. Enormous blocks of water thus stand in immovable columns, surmounting the loftiest pinnacles of our globe. How different are these from the soft insinuating liquid, which is the circulating medium of all organic life!

" Every common water in its natural state, whether salt or fresh, contains about a fortieth of its bulk of air, which it readily evolves either by the heat of ebullition, or when placed in vacuo. In these circumstances, we perceive minute globules oozing out from every point of the liquid, which when liberated, rise by their innate lightness and elasticity, in a pearly-looking stream to the surface. Now this liquified air is the element of respiration, the *pabulum vitæ*, to all bronchial animals. The gills of fish elaborate that liquid air, as the lungs of land animals do its elastic form. Both organs convert its oxygenous part into carbonic acid, for the sustenance of vegetation, terrestrial or submarine.

" Enough has now been stated to evince, that water in its liquid state is not the uninteresting, inert element which the multitude suppose it to be, but that its constitution is most refined and intentional, adapted to the manifold functions which it must discharge towards countless orders of organic and inorganic being. Its habitudes with heat are peculiarly beautiful. A certain energy of this power gives to water its liquid condition, in which, as already mentioned, the molecular attraction is very nearly neutralised by the repulsive force. Had the cohesion of its parts been less, it could not have afforded the *reaction requisite* for the movements of fishes and ships; nor could man have availed himself of its impulsion to aid his feeble arm in preparing his food, clothing, and domestic accommodation. The same calorific agency wafts the ponderous liquid on the wings of the wind, to supply the organic inhabitants of mountains and plains with vivifying moisture; or under the guidance of mechanic genius, it animates the steam-engine, the noblest offspring of science and art, the unwearied and docile servant of man.

" The equilibrium of its mobility lies within narrow bounds. A very moderate reduction of temperature restores the cohesive power to uncontrolled dominion, under which the form of water ceases to exist, and might thus continue unknown. Again, had the particles of water been mutually elastic, though but in an inconsiderable degree, the least disturbance by winds or tides must have produced tremendous commotions in its mass; and a floating body would have been alternately tossed into the air, and plunged into the depths, with destructive violence. Under such circumstances, the sea would have been an impassable gulf, between closely adjoining kingdoms, instead of being, as now, a thoroughfare between the most distant lands, the element of the noble art of navigation. By its almost inelastic and incompressible *crasis*, the body of the ocean cannot resile, nor suffer agitation beyond a few fathoms from its surface, even from the force of the hurricane. In fine, the absolute passiveness of water, and independent gravitation of its parts, form its most valuable qualities."

Chapter V. is occupied with primitive rock formations. Our author has here very properly availed himself of Dr. Macculloch's excellent papers on granite, published in our old series of the Quarterly Journal of Science—referring on this, as he does on all similar occasions, to his authorities. He has also turned to good account Von Buch's late observations on eruptive rock phenomena in the Tyrolese Alps.

We shall quote a paragraph from this chapter, to show our author's manner of treating the subject.

" The primitive envelope of the globe seems to have originally consisted of concentric strata of gneiss, mica-slate, and clay-slate, with partial layers of semi-crystalline limestone; for such, with a few inconsiderable exceptions, constitute its rocky crust, and are spread over all its regions. These coats, however, no longer lie in layers concentric with the spheroid, but are thrown up into nearly vertical planes, and transpierced in many points by towering masses of granite and porphyry.

" On the primordial spheroid covered with its illimitable ocean, these stratiform coats lay in horizontal planes; but with the gathering together of the waters, on the emergence of the land, they were heaved up abruptly into the nearly vertical tables, in which they now universally stand. This remarkable position corresponds to the eruptive violence that caused it. From the shoulders and flanks of the stupendous granite peaks, mantles of gneiss and mica-slate depend in magnificent drapery. These schistose coverings are arranged near the summit in folds almost upright, which lower down, become sloped off with clay-slate and limestone into a gentle declivity. The coats of gneiss are often contorted into the most singular flexures of rock scenery, demonstrating a certain pliancy of texture at the instant of erection; resulting either from the moisture out of which it rose, or the softening influence of subterranean fire.

" The wood-engraving at the bottom of this page, taken from D'Aubuisson, represents the usual structure of the primitive mountains. It is impossible for an unbiassed mind to contemplate this sketch, given by a Wernerian geologist, without seeing its entire conformity with the eruptive mode of formation."

Book Second is entitled the *Antediluvian Period*, and includes all mineral formations which contain any vestiges of organic life. It is divided into five chapters, which present a succinct description of the great mineral planes, from greywacke up to the diluvial gravel. Here he has introduced, in their appropriate places, lively abstracts from Cuvier and other writers on the organic remains, found among the secondary and tertiary strata. These are illustrated by well selected wood-cuts. This portion of his work will contribute essentially to promote the popular diffusion of geological science.

The following view of the Paris basin, may be cited as an ingenious attempt to solve a geological puzzle.

" Such is a brief transcript of the admirable memoir of MM. Cuvier and Brogniart, on the most interesting formation hitherto explored by the joint resources of geology and physiology. It exhibits an unfading picture of the convulsions which the primeval globe suffered a few centuries prior to its close. The chalk beds of both the Paris and London basins evidently stood, at first, near the level of that ancient ocean, as may be deduced from the billowy undulations on their surface. Had they lain deep under water, the rolling of the waves could not have chequered their face with those remarkable ridges and excavations, which still exist to indicate the cause of their origin.

" It was about this geological epoch, that some of the great fields of basaltic-lava were poured forth from the interior of the globe, spreading over the secondary strata of chalk and sandstone, as we see exemplified in the north of Ireland, in Scotland, and many other countries. The epoch of the covering over of the great chalk formation of Antrim with basalt, coincides with the covering of the chalk basins of Paris and the Isle of Wight, with their tertiary strata. The acts of eruptive violence which immediately clothed the submarine chalk surface of one region with an immense sheet of lava may have upheaved the chalk slightly above the level of the sea in another district not far distant, thus converting a marine basin into an estuary of great extent, which probably comprehended at once the London, Isle of Wight, and Paris basins. For the depression between them that forms the bed of the English channel is very trifling in proportion to its extent of surface, and is very much exceeded in depth by the large continental lakes.

"2. The estuary cut off from the sea by alluvium at its mouth, would become progressively fresh by the influx of river streams, and change into a lake, on whose bottom, clay, vegetable remains, and freshwater shells would be deposited.

"3. But as the phenomenon of trap veins transpiercing one another proves that a series of submarine convulsions took place at successive periods, a second eruption of basalt might inundate the lake for a season with a shallow body of sea-water, introducing with it a colony of new inhabitants. By successive deposits of their exuviæ, along with calcareous matter and sand, the bottom would eventually rise above the tide level; and the sea would thus be slowly shut out, as the flood-gate became more complete; the production of sea-shells would proportionally diminish, and cease entirely, as the sea withdrew, and the ground became once more, first a brackish marsh, and then a fresh-water lake. 'Peu à peu cette production de coquilles diminue, et cesse tout-à-fait; la mer se retire, et le sol se couvre de lacs d'eau douce.'

"4. Now were formed the solid deposits of marly limestone, as in the *travertino* of Italy: now the gypseous waters let fall their precipitate of soft plaster, which gradually consolidated round the bones of the *anoplotheira, paleotheira, birds, crocodiles, trionyx tortoises, palms*, and other productions of the tropical climate which then obviously must have prevailed in the latitudes of London and Paris. The period of the deluge was now drawing nigh, and partial dislocations of the strata began to betoken that awful catastrophe. The ocean bed, once more upheaved by a submarine expansive force, caused its waters to surmount the embouchure of the Parisian basin, and make it again an estuary of the sea.

"5. Now the third marine formation of Brogniart, or second above the chalk, commenced, with its yellow clay marls, its sandstone and marine sands, its marl limestone and upper sea marls. These deposits, on reaching a certain height, necessarily became a barrier to the sea, reconverting the basin into a fresh-water lake.

"6. In this state it continued, till, by successive depositions, and the drainage of the waters after the deluge, it was finally brought into the existing condition.

"Thus the vicissitudes of the land and ocean, pourtrayed in the tertiary formations, harmonise perfectly with other terraqueous phenomena of the same geological period. The whole may be regarded as characteristic preludes of that storm which ere long destroyed the old world and its inhabitants."

The *Third Book* is called the *Deluge.* It contains a great variety of curious information, as we may infer from the following list of its Chapters:—

Chap. I. Physical records of an universal deluge, which new-modelled the earth.—Chap. II. Causes of geological catastrophe, distributed into two sections, of volcanic action and basaltic eruption.—Chap. III. The constitution of the primeval world, and the revolutions which it underwent, deduced from geological phenomena on physical principles.—Chap. IV. Elevation of submarine strata.—Chap. V. Phenomena of the deluge.—Chap. VI. Animal remains or ruins of the deluge.—Chap. VII. The present earth, and era of its emergence.

We regret that our limits will not permit us to give so many or such lengthened extracts from this portion of the work as we could wish. We must confine ourselves to an outline of his theory of the primeval climates; presenting views which we believe to be equally new and striking.

"From the evidence advanced in the preceding chapter it incontestably follows, that a vast magazine of fire and explosion lies immediately within the crust of our sphere, throughout its whole terraqueous zones.

"The subject of the interior temperature of the globe has been lately investigated with all the resources of mathematical analysis by M. Fourier."

. . . . . .

"The preceding facts, and other similar ones, concur to show that the increase is nearly a degree of Fahrenheit for 65 feet. This increase will not be always of the same amount as at the present day: it will diminish progressively; but a great many ages must elapse before it be reduced to the half of its actual value. The extent of this diffusion of the central heat into the circumference, and of its waste into the celestial spaces, will therefore be proportional to its primitive intensity, and to the conducting quality of the investing materials.

"If we apply heat to the flat bottom of a deep vessel, (of iron, copper, &c.,) which contains several alternate layers of sand, clay, and stony slabs, condensed as in the supermedial strata of England, and covered with water, we shall wait in vain for any distinct manifestation, at the top, of the subjacent fire: in fact, the lowest layer will become compacted by the heat into a schist impervious to liquids; so that the incumbent water will never arrive at the calorific source, and, severed by bad conducting matters, can never grow appreciably warm. In the great boilers of steam-engines, many results to this effect daily occur, which form sources of very serious annoyance. Wherever the waters of supply are calcareous, more especially selenitic, they let fall a crust of gypsum on the bottom, which progressively thickens, so as to intercept a large portion of the subjacent heat, and by separating the iron from the water, allows the metal to become ignited, and to burn away. Such a deposit has been known to grow several inches thick, with a stony hardness; and, till laboriously chiselled off, it has rendered the vessel quite inoperative for raising a due supply of steam.

"We have merely to compare these incontestable results of art to the kindred phenomena of nature, to recognise not merely the analogy, but identity of operation in the two cases; for difference of magnitude constitutes no disparity of essence.

"In the early epochas of the antediluvian world, soon after the granitic atlas had uplifted the primitive mountains, and before the extensive series of mineral beds, which occupy our second book, were deposited beneath the ocean, its waters resting on the nearly concentric, or slightly broken zones of gneiss and mica slate, necessarily lay in closer proximity with the interior fires than at any subsequent period. Hence two important consequences: 1. From the thinness of the solid crust, the smallest chink or fissure in it would be an immediate focus of submarine explosion, accompanied and followed by a commensurate comminution and dispersion of the solid rocks and organic deposits through the agitated waters. 2. The ocean would then attain its maximum temperature; a pitch certainly far higher than at present, yet not incompatible with the vital functions of fish, many of which, according to Humboldt, can live in water almost boiling hot. Desfontaines found the *sparus* thriving in tepid fountains of 100° Fahr. near the town of Cassa in the kingdom of Tunis.

"From the extreme mobility of its molecules, water is the most expeditious conveyer of heat from below upwards; while, from its *non-conducting* quality, it is a most faithful carrier, losing none of it during its ascent. Hence any degree of warmth, however gentle, imparted to the bottom of the oceanic mass, will be transmitted unimpaired to the surface. And again, as water possesses a very high specific heat, one four times greater than air by weight, so that five gallons of water in cooling only one degree F., can warm by the same quantity 2650 cubic feet of air, being the contents of a chamber about 16 feet square, and 10½ feet high; we see what a genial climate would be created over the earth from pole to pole, under such an order of things. Then the intrinsic source of terrestrial heat, having its diffusive energy but slightly obstructed, would be paramount over the solar; so that the position of the sun, relative to the equator, would act a very subordinate part in modifying climate, instead of being its sovereign arbiter, as at the present day. Plants which love a warm but humid atmosphere, like the equisetums, ferns, &c., would multiply and flourish, under such circumstances, with nearly equal vigour in the Arctic regions as under the Line. Hence also the difference of equatorial and polar temperatures would be at first comparatively small, so that a considerable uniformity of vegetation would pervade the most distant zones. We need not, therefore, be surprised at finding the same *Calamites* or gigantic equisetums buried among the coal-measures of New Holland, (near Port Jackson,) and of England; though now-a-days, that plants are subjugated to the undivided empire of the sun, they differ in species with very moderate variations of latitude, and with every change of hemisphere.

"The first age of the world then, extending probably through several centuries, fully realized the universal and unfading spring of the poets. Under such fostering powers of vegetation, the coal-measure plants were matured, in countless myriads, with a rapidity to which modern experience can furnish no parallel. But the tremendous catastrophes of the crust of the earth, that took place soon after this period, of which the dislocations and disruptions of the coal-strata themselves exhibit magnificent memorials, generated a vast quantity of detritus from the older rocks, which at first diffused through a turbid ocean, progressively subsided on its bottom in the chemical order of deposition; constituting beds of conglomerate limestone, red marl, and lias; in variable proportions of thickness and extent, according to the nature of the exploded and comminuted rocks. In the secondary formations of geology, in fact, we see nothing but a repetition of mineral *triads;* shells more or less fractured, covered with a two-fold coat, the undermost of sand or sandstone, the uppermost of clay more or less indurated. The tepid ocean-bed vied in fecundity with the glowing soil round its shores, and thus was covered with a thick deposit of shell-fish and their exuviæ. At each rencounter of the water and subjacent explosive metals, these shells would be more or less scattered and broken down, and when tranquillity returned, covered with their siliceous and argillaceous mantles.

"The conglomerate limestone and red marl are referred by geologists (see Conybeare and Phillips) to the detritus of the primitive and transition rocks; deposits just posterior to the coal formation. It is probable that the submarine disturbances of that particular age were unfavourable to the multiplication of *mollusca*. But a period of repose seems to have followed, in which the shells of the lias were elaborated. These, with a little alumina, are condensed into the lithographic stone, and buried under a loamy compound of sand and clay. We have next the inferior oolite; merely a congeries of pulverised shells, roofed with the corn-brash, &c., and overlaid with the Oxford clay. Then we come to the coral rag, teeming with vestiges of vitality; inhumed also beneath its sheet of Kimmeridge clay.

"When we arrive at the Portland strata, we must consider that a series of most imperfect conductors of caloric, fully half a mile in thickness, had been by this time interposed between the bottom waters of the sea, and the deeper primitive or transition crust, on which they originally reposed.

"The climate of the earth should therefore indicate, about the Portland era, an abatement of the hypertropical temperature of the first age."

. . . . . .

"In addition to the results of the successive interpositions of non-conducting media, between the ocean and its subjacent fires, in lowering the surface temperature of the globe, we must take the following phenomena into account. By each series of explosive dislocations of the terrestrial crust, the area of the 'land standing out of the water' would be abridged; that of the sea would be amplified, with a proportionate diminution of its depth; or, in other words, the cooling surface would be augmented at the expense of the heating surface; while the ocean would come to repose, as we have seen, on a cooler bed, because more distant from the central heat of the earth. These propositions we shall endeavour to place in a clear and certain light by an ample induction of facts.

"In a revolving terraqueous sphere, deviating from the equilibrium form of rotation, by its elevated lands and deep ocean beds, at every considerable disruption and comminution of its surface, the gravitating powers will become effective on the shattered shell, and arrange its fragments, so as to make the crust approximate more nearly to the geometrical spheroid. The mountain and table-land masses will thus be strewn over the concave bottom of the seas, and cause a new distribution of the waters round the sphere; in which the area of the dry land will be diminished proportionally to the extent and duration of the disruptions.

"Supposing the earthquakes, and consequent comminution of the shell, not partial and successive, but universal and synchronous, then the whole crust of the earth having its cohesion destroyed for a season, would forthwith obey the gravitating forces on a revolving sphere, would assume the spheroidal figure of rotation, and remain universally circumfused with water, as under the primordial abyss. But a partial and successive series of disruptions of the crust will cause only a partial approximation to that ultimate figure, accompanied with a transient deluge of greater or less extent over the surface. When the explosive commotions cease, the ocean undulations will subside, the sea will flow back into larger but shallower basins, and the dry land will again appear, furrowed, and strewed over with the detritus of the storm. This view of a diluvial transition, in my apprehension, can hardly be deemed hypothetical, resting as it does on the joint bases of geological facts and physical laws."

. . . . . .

"We have therefore every physical reason to conclude that each great antediluvian convulsion of the earth extended the empire of the sea, and abridged the boundaries of the land by a permanent submersion of some of its regions; mechanical effects involving commensurate physical changes of climate; 1. by the thickening deposits of the ocean; 2. by the increase of the cooling or aqueous surface of the globe; and 3. by the decrease of the heating or terrene, as will be fully developed in treating of the deluge in the fifth chapter."

These novel views are resumed, with additional explanations, when the phenomena of the deluge are described.

"The *bouleversement* involved in these propositions, and deducible from the phenomena on which they rest, must, by the established laws of physics, as we have already shown, have materially increased the area of the ocean, and diminished that of the land. To what degree this change of proportion amounted, we have no precise data to determine.

"On Mr. Penn's principles, the ratio of land to water was inverted by the deluge; for he assumes that our actual seas correspond in surface to the antediluvian lands, and our actual lands to the antediluvian seas. But the researches of Professor Buckland on the Kirkland and Franconia caves, as well as those of Baron Cuvier on the grotto of Oiselles, concur to prove that these were dens inhabited by antediluvian quadrupeds; and therefore must have formed a portion of its dry land. Moreover, most of our coal districts, and primitive schistose mountains, in Scotland, for example, bear no good evidence of having lain under the sea during the long antediluvian period. A few shells may no doubt be found scattered over their surface, relics of the deluge; but these marine exuviæ do not constitute regular testaceous strata in their body, as would undoubtedly have happened during a long residence in the bosom of the ocean. Our coal-measures, indeed, are most probably the basins of antediluvian lakes and marshes.

" With Mr. Penn's proportion of land and water, I conceive the terraqueous globe would not have been habitable by man and his companion animals. It would have possessed nearly three parts of earthy surface to one of aqueous; whereas there is now fully three of aqueous surface to one of earthy. Or, since dry ground is the heating surface, and water is the cooling, the heating faculty of that ancient globe would have been three times greater than the present, and its cooling faculty three times less; making a nine-fold difference in calorific constitution between the two, without taking into account the proper heat of the antediluvian seas. Under such circumstances of heat and aridity, vegetation must have pined, or most probably expired, except in a few narrow bands of soil along the margin of the sea; as is now exemplified on the Barbary shores. If we suppose that only one half of the primeval land *perished* at the deluge, and *that* half possibly a great continent, (corresponding to the Pacific Ocean,) which might form the whole world to the antediluvians, unversed as they evidently were in navigation, then the area of their dry land would have been equal to that of their seas; and the general climate of their globe, as far as depended on the constitution of its surface, would have been three times warmer and drier than the present. This by no means implies, however, thermometric and hygrometric degrees three times higher than the present. Besides, the hotter surface of the land would be compensated by a greater radiation of heat into space, and the hotter surface of the seas by a more copious evaporation of water into the air. Still that incontestable physical principle will perfectly account, to a certain extent, for the higher temperature which prevailed in our latitudes in antediluvian times, and for the sudden and vast refrigeration induced by the deluge. I do not presume to define the numerical proportions of land and water on that ancient globe; but I maintain that such a mighty diluvian catastrophe, as it suffered, could not be accomplished by any powers of nature which the laws of inductive logic authorise us to employ, namely, general explosive and disruptive forces of volcanic origin, without increasing the area of the sea, at the expense of the land. We must moreover keep in view the increasing obstruction of the central heat after each successive catastrophe."

The specimens now exhibited of Dr. Ure's work will recommend it more effectually to the notice and patronage of our readers than the most elaborate eulogium which a critic could indite. The wood-cuts and plates represent a well-selected variety of mineral sections and organic remains. In five copper-plates are figured seventy fossil shells, characteristic of the several great strata from the mountain limestone up to the London clay. There is an exquisite lithographic impression of the petrified stem and leaves of a cactus cylindricus—a fossil which appears to occur very plentifully in a coal sandstone in Ayrshire.

The popularity of Dr. Ure's work will be deservedly enhanced by the consideration that he has triumphantly confuted every cavil of the least consequence, which geological sciolists have urged against sacred history. And all this is done in a truly philosophical spirit without the slightest air of religious intolerance. On the whole, we regard this new system of geology, as one of the most valuable accessions lately made to the scientific literature of our country.

On some Phenomena of Vesuvius.—Extracted from the Journal of, and communicated by OCTAVIUS MORGAN, Esq.

March 25th, 1828.—Vesuvius had all day been sending forth, at very short intervals, immense volumes of dark smoke, or rather dense vapour, charged with ashes and sand, which were carried in an eastward direction, by a strong westerly wind that prevailed all day, and which fell in showers on the opposite side of the mountain, as was plainly discernible from Naples, darkening the air beneath as they passed. We started from Naples at 7½ P.M., and in our way to Resina, and afterwards, whilst ascending the mountain, I saw frequent flashes of sheet lightning, some above the volcano, and others in various directions all around—not seeming to proceed from any particular spot. There was also much hail both on the cone, and down below at Resina. We ascended to the summit without other inconvenience than a very strong wind. On the brink of the crater, the wind was so tremendously powerful that it was utterly impossible to stand; to creep on all fours was our only chance of escaping being precipitated into the crater. The coldness and impetuosity of the blast, and the quantity of ashes and sand swept by it from the edge of the cone, not to omit a most violent hailstorm, rendered our situation peculiarly unpleasant. The crater was by this time nearly half filled with the scoriæ which had been thrown up from the liquid lava beneath, and floating on its surface had thus accumulated. The surface was funnel-shaped, and from the central depression, the explosions and ejections usually proceeded, the effect of which was, as if a very sudden and violent blast, or disengagement of vapour, rushing up from beneath, carried up with it into the air a quantity of these red-hot masses of scoriæ, raising them to a great height above the crater, as though they had been no heavier than feathers. The appearance of the crater at this time was as an enormous furnace, or a valley of fire, the surface of the scoriæ being nearly black, except at the moments of the explosions, when the whole glowed with the most vivid brightness. The central depression was always at a white heat. Whilst we were sitting on the brink of the crater, a new spot, nearer to us than the old point of discharge, burst forth within the crater with a prodious explosion, the mountain trembling beneath us. A vast quantity of red-hot scoriæ were hurled to an immense elevation, and scattered about on all sides. The violence of the wind, however, drove them from us. As I sat behind the other persons, I observed the following curious phenomenon. On the highest parts of the hats, coats, cloaks, hoods, &c. of all those before me, was a phosphorescence, that is to say, a pale greenish light was visible on the most elevated extremities of all parts of their clothing that were exposed to the wind. A green crape veil, (which I wore to protect my face from the sand and ashes carried by the wind,) when agitated by the blast, appeared to be edged with a fringe of sparks, resembling the light of a glow-worm. When removed from the influence of the wind, the light ceased; but on allowing it again to float in the gale, the light instantly reappeared. The tips of my gloves on being exposed to the wind bore each a spark, like a glow-worm's tail, and seemed communicable from one finger to the other by contact, not unlike phosphorus. Lower down the mountain, this appearance was not perceivable, though exposed to the same wind. When we left the crater*, the wind and hail were so violent, that it was impossible to keep our torches alight, and we were obliged to grope our way back to the Hermitage in the most complete darkness, over the broken lava, perhaps the most rugged spot on the face of the globe.

ON RADIATING ELECTRICITY, *by* Charles Bonnycastle, Esq., *Professor of Mathematics in the University of Virginia in the United States of America. Communicated by* P. BARLOW, Esq., F.R.S.

WHEN BENNET first invented that beautiful little instrument, the gold leaf electrometer, he found that a candle placed upon the top of it greatly increased its sensibility; and other experimentalists were not long in observing that a candle possesses the property of depriving all bodies in its neighbourhood of the electricity imparted to them. About a year ago I made some experiments upon this subject, and arrived at what appeared to me an important fact, that electricity radiate, with great velocity when the body upon whose surface it exists is raised to a *white heat*. I was much struck with this results from its analogy to what I had before witnessed, when assisting Mr. BARLOW in his researches on the magnetism of hot iron. We had found the magnetic action to cease entirely at the moment when the temperature became that of white heat, or somewhere about 1000 degrees of Fahrenheit; and were at a loss to explain the phenomenon, which I have now little doubt arose from a radiation of the magnetic fluid. A similar analogy occurs in galvanism; the brilliant development of light and heat, which takes place when the connecting wires of a powerful battery are armed with charcoal points, does not commence until the charcoal has become intensely white; but it will afterwards be maintained, although the points are removed several inches asunder, and from all the phenomena, is manifestly a case of radiation.

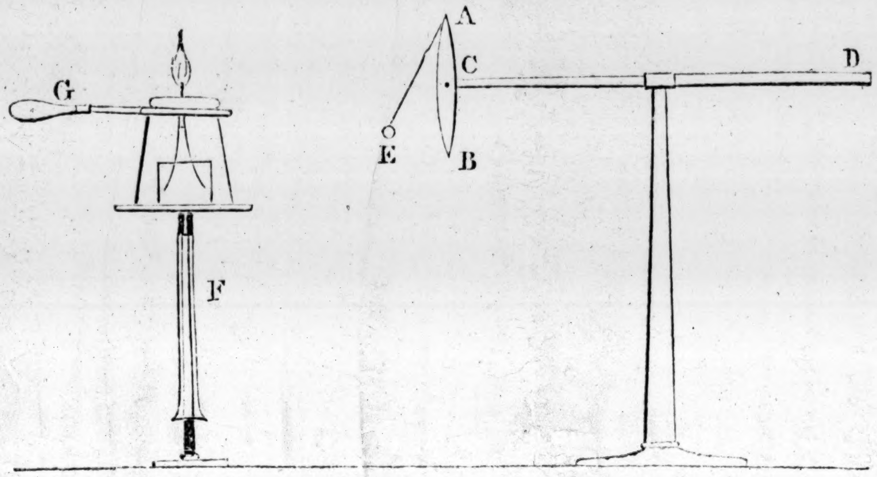
---

* It was about half-past twelve o'clock. It occurred to me at the time that it was a powerful discharge of electric fluid; but how excited, or by what laws thus discharged, I could not discover.

Thus, if the view I have taken be correct, it follows that *electricity, galvanism, and magnetism are radiant fluids ; that they are caused to radiate by heat, and commence at a fixed temperature, which is precisely that at which light itself is first given off in abundance.* This law is another instance of the connexion between light, electricity, galvanism, and magnetism, and seems to point out that heat is the universal agent by which repulsion is effected. I have added to the experiments on which it is founded, some remarks upon the theory of electricity when in a state of equilibrium, tending to shew that a condition is requisite which Poisson and Biot have not explicitly mentioned, and which serves as the connecting link by which we pass from the properties of the electric fluid when in a state of quiescence to those it possesses when radiating. I have likewise, with the view of removing a difficulty in electricity, given a practical method of causing the spark to pass in either direction. Though the facts which I have mentioned are all drawn from phenomena that are familiar to the electrician, they appeared to me to have been neglected, or misunderstood, which led to the experiments that I shall now detail.

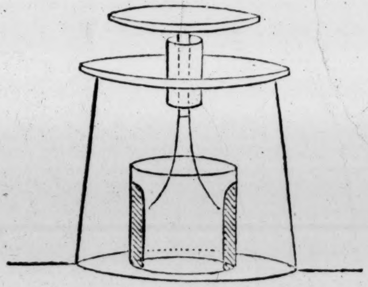
As it was evident that error might arise from a want of perfect insulation, and from the air getting charged with electricity, I made use of very low charges, and as small and simple an apparatus as possible: at first this consisted only of an electrophorus of one foot diameter, with an insulating stand; a polished gilt button, hung by a single thread of unwrought silk about two feet long; a candle; and a gold leaf electrometer. The last I found it advantageous to construct a little differently from that commonly in use. It was formed of a glass vessel, about three inches high, and two and a half wide: closed at the top with a small plate of talc coated in the middle with tin foil, and from which hung the strips of gold leaf. The sides were not as usual coated with tin foil, but a smaller cylinder, coated in this way, and open at each end, was introduced under the first, and received the electricity from the leaves when they were expanded. See fig. 2.

Fig. 1.



This arrangement renders the use of the instrument more convenient; whilst the second glass is in its place, the electrometer is as accurate as that commonly used; and when this glass is removed, the leaves may be made to open permanently, whilst we examine the electricity with which they are charged; without fear of their sticking to the glass, or of their flashing and discharging themselves before the necessary examination has been effected. The plate of talc which forms the top of the instrument, is liable to get slightly excited or charged, and therefore, where great accuracy is required, it is better to have the top of gum-lac, and to use the mode of insulation recommened by SINGER.

Fig. 2.



Ex. 1. The small plate charged from the electrophorus was held at an inch and a half from the flame of the candle for one minute; when it was immediately carried to the electrometer, and held near to the coated plate. The whole of the electricity had disappeared.

Ex. 2. The charged plate was held over a mass of iron weighing several pounds and heated to a dull red. The time as before, one minute; when removed to the electrometer, the plate appeared not to have lost any of its electricity.

Ex. 3. The plate was held over red-hot charcoal, some of the pieces of whch were at a white heat. Lost its electricity.

Ex. 4. As it was conjectured that the small pieces which were white hot might have produced the effect, the plate was held over a similar mass of charcoal, no part of which was above a dull red. No loss.

Ex. 5. The plate was held over various flames, over the flame of sulphur, of hydrogen, over platinum rendered white hot by a current of hydrogen gas, and finally over the flame of alcohol; the result in every instance, excepting that of the sulphur, was a total loss of electricity. The flame of sulphur deprived the plate of its electricity when it was sufficiently intense to approach whiteness, but when the combustion was confined to a small spot, and the flame was perfectly blue, it had little effect in abstracting the electricity of the plate.

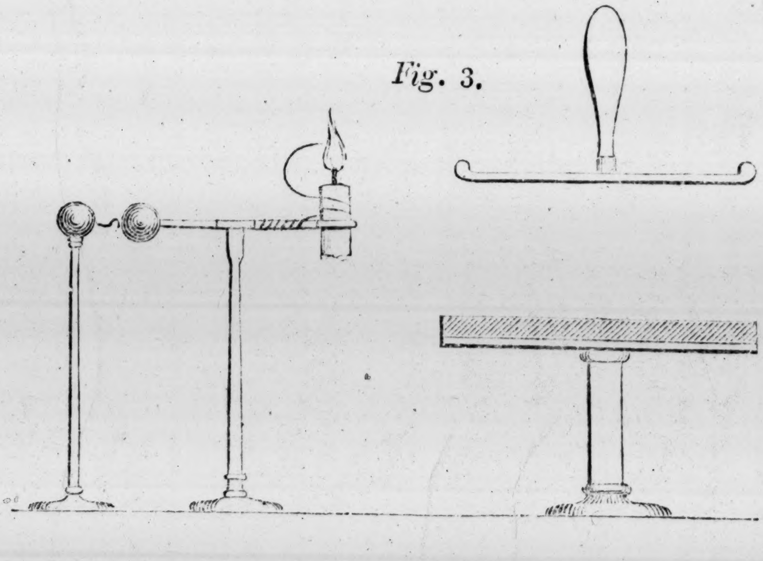
Ex. 6. To determine whether a limit with regard to extent of combustion existed in all bodies as well as sulphur, or whether in the latter a small portion burnt less intensely than a large one, the plate was held over a minute piece of charcoal, a point upon which was kept at a white heat by a blow-pipe. The loss of electricity was total. This experiment is best performed by placing a piece of charcoal about the size of a pea on the plate of the electrometer and hanging the small charged plate over it; whilst the charcoal is nearly extinct no effect is produced, but the instant a brilliant point is produced by the blow-pipe, the leaves of the electrometer diverge, and remain apart: and the whole of the electricity will be found to have left the plate.

Ex. 7th. Robison and others have explained the depriving power of points to a stream of electrified air which they maintain; to discover whether the depriving power of flame might be produced in a similar way, the plate was blown upon by bellows, for four seconds, without any material loss of electricity, which was likewise the result when it was plunged in the vapour of boiling water. From this last experiment we may observe how small a part of the loss which takes place in all electrical experiments arises from the conducting power of the air, even when charged with moisture; nearly the whole must be attributed to the imperfection of the supports.

It was now sufficiently obvious that combustion, or a temperature sufficient to render most bodies luminous, was requisite to give this power of abstracting electricity at a distance; whilst the hypothesis of a current of air appeared insufficient to explain the operation by which this abstraction was effected. Before any other could be framed, it became necessary to examine what took place at the light, whilst exerting its abstracting power. After a little reflection, the following experiment was devised for this purpose.

Ex. 8. A candle was insulated by being placed in a ring formed at one end of a wire, which slid through the ball of an insulated pillar; the other end of the wire terminated in a ball *, and was brought within a quarter of an inch of a similar ball which was connected with the ground; a smaller wire connected the flame of the candle with the ring. About twelve or fourteen inches from the candle and opposite to it was placed the charged electrophorus with the plate down; the arrangement will be understood by a reference to *fig.* 3. Immediately

Fig. 3.



---

* One of the wires of Lane's discharger answers perfectly well.

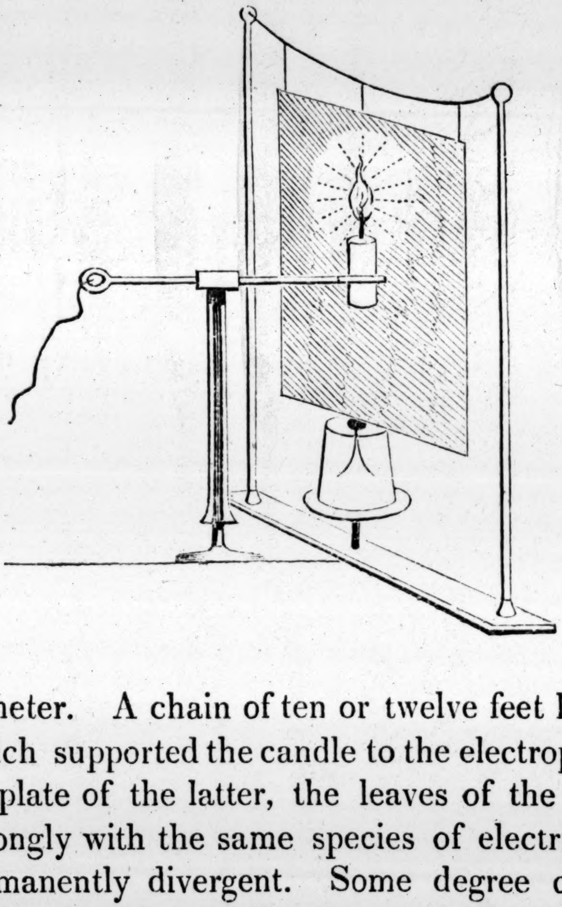
the plate of the electrophorus was raised, sparks were seen to pass between the insulated ball and that which was connected with the ground, and continued to do so at intervals until the plate was nearly discharged. The candle being removed and a sharp point substituted in its place, on repeating the experiment, the abstracting power of the point was found to be insensible at this distance.

Ex. 9. The former experiment was repeated under another form, which admits of examining the nature of electricity produced at, or conveyed to, the light. The disposition of the apparatus is as follows: A metal plate A B, *fig.* 1, (p. 136) was screwed to a glass tube C D, three feet long, and half an inch diameter. The rod slid through a stand, fourteen or fifteen inches high; and the plate was furnished with a pith ball electrometer E. The gold leaf electrometer, F, with its coated guard, already described, was placed eight inches from the plate, upon an insulating stand. Lastly, a small spirit lamp, of metal, and free from sharp points or edges, was placed upon one of the plates used in BENNET's method of condensing. When every thing was ready, the plate A B was charged until the ball stood at 45°, and immediately afterwards the lamp, by means of its insulating handle, was put on the top of the electrometer. At the instant of contact the leaves of the electrometer flashed and discharged themselves at the guard, and continued to do so for a quarter of a minute. During this time the ball E continued to descend, and at the end of it the plate was completely discharged. Towards the latter part of the experiment, when the motion of the leaves was slow, their electricity was examined by a proof plate, and found to be of the same kind with the electricity abstracted.

These experiments demonstrated that the depriving power of combustion arose either from an actual abstraction of the electricity in the bodies within its influence, or from a radiation of the opposite electricity proceeding from the flame. The results, including even the nature of the electricity in the last experiments, could be explained on either supposition; but the following experiment was more decisive.

Ex. 10. A candle, insulated as in the 8th experiment, was placed sixteen or eighteen inches from a screen of tin foil: the latter was one foot square, and hung by silk threads as in *fig*. 4. The lower part of the foil was in contact with a gold leaf electrometer. A chain of ten or twelve feet long led from the wire which supported the candle to the electrophorus. On raising the plate of the latter, the leaves of the electrometer diverged strongly with the same species of electricity, and remained permanently divergent. Some degree of divergence was produced when the candle was not employed; but in this case the leaves collapsed immediately the electrophorus was closed, and the nature of the electricity possessed by the leaves depended on the position of the electrometer, that is, whether the leaves were before or behind the sheet of foil. It could not be doubted, therefore, that a real transfer of electricity took place in the first instance, and that merely induced electricity was present in the second.

Fig. 4.



Ex. 11. The last experiment was repeated; but in place of the sheet of foil, a disc of silk of three inches diameter, stretched upon wire, and insulated by a pillar of glass with a

ball and socket, (*fig.* 5) was employed. By turning the socket, this disc could be placed in any position with respect to the candle, and several experiments were made with it in front, behind, on the side, and above the flame. The electricity stopped by the silk was examined by taking the disc to the electrometer; and was found in all the instances to be positive, and nearly of the same intensity; rather less electricity, however, seemed to radiate downward than horizontally.

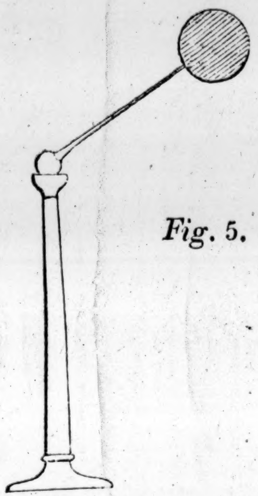


Fig. 5.

Ex. 12. As the radiation in these cases was undoubtedly from the light, it seemed fair to infer that such is the case when the combustible is not itself electrified, but is exerting its abstracting influence on other bodies. Were this the case, it seemed to follow that a screen interposed between the two ought to receive electricity from the body in combustion only, and consequently would be in an opposite state to the charged body. An arrangement was accordingly made similar to that in the 9th Ex. (*fig.* 2), but a screen of glass one foot square, and a quarter of an inch thick was interposed between the charged plate and the condenser: the result was remarkable; immediately the light was placed on the electrometer, the leaves of the latter began to diverge, and continued to rise and discharge themselves against the guard at intervals of nearly a minute; whilst the ball E fell as slowly as if no light had been employed. When this was actually the case, the same phenomenon was observed; but it was found that the leaves whilst diverging were now charged with negative electricity, whilst, when the light was used, the electricity with which they diverged was positive. This difference was readily explained, and was consistent with the theory adopted. When the electrometer was exposed to the influence of the charged plate, with or without an interposed screen, but without employing a candle, the electricities in it soon became neutralized, and the leaves hung parallel: after a minute or two, the plate having lost positive electricity, the negative electricity in the electrometer was no longer neutralized; the excess in the guard readily escaped, but the leaves being insulated, the excess was confined in them, and caused them to diverge with negative electricity. That such is the true explanation was rendered manifest by suddenly discharging the plate, when the leaves instantly flashed and discharged themselves. With the candle the effect was reversed, because, in this case, negative electricity radiated from the electrometer; and this radiation produced little or no effect on the charged plate, because it was intercepted by the glass screen. When the latter is very large, or the plate is inclosed in a glass case, I have found both the dissipation, and the effect on the electrometer, to be reduced to little or nothing; and this too is consistent with the hypothesis: for the screen will itself become charged with induced electricity, and will neutralize the effect which the charged plate could otherwise have had on the lamp; as may be satisfactorily shewn by charging a plate of glass coated on both sides: so completely does the negative side neutralize the positive, that if *this* side be held over a gold leaf electrometer, it would produce no effect on the leaves, although the plate may contain electricity enough for a strong shock. The same action of the induced electricity in the screen which rendered that on the light feeble, interfered with the result which had been anticipated respecting the charge which the screen would take, both by causing the light to radiate less electricity, and by the induced electricity, which does not immediately recover its equilibrium in a non-conductor, mingling its action with that of the radiated electricity. I have since, however, removed this difficulty by interposing the small silk screen, *fig.* 5, between a lamp, and the prime conductor of an electric machine, whilst the latter was in action; the screen, as anticipated, became charged with negative electricity.

So powerful is the effect of a lamp in abstracting electricity, that a large jar may be readily charged when six inches from the prime conductor of an electric machine, by placing a lamp on the cover of the jar. In this experiment the charge will not increase beyond a certain degree, and if the radiant electricity be then examined with the small silk screen, it will be found to be positive on one side of the light, and negative on the other; so that one part of the flame is charging the jar, and the opposite discharging it. A jar with a foot and a half of coating was charged in this way to ten or fifteen degrees of the quadrant electrometer, when two feet from the prime conductor of a seven-inch cylinder. If the light be inclosed in a metal vessel, leaving only communication enough with the atmosphere to sustain the flame, the jar will still be charged, though to a less intensity; but if the vessel be of glass, the jar will not be charged at all, and the glass envelope will become strongly negative. A jar will not be completely discharged by a light placed on the top of it whilst the air is still, but a current of air facilitates greatly both the discharging and abstracting power of combustion.

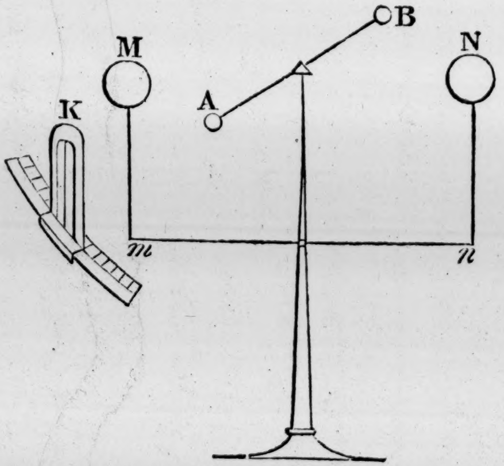
The phenomena which I have described will be explained by assuming, that a body in a state of combustion has the property of impressing on either of the electric fluids, when flowing through it, a force of radiation which enables them to penetrate many feet of atmospheric air. When a lamp is placed on an electrified body, the electric fluid passes to the light, and is thence radiated into space. But as the effect ceases before the body is completely discharged, we must further assume, that the radiated electricity adheres to the particles of the air, and forms round the lamp an atmosphere of electricity which by its repulsion prevents any further radiation from taking place. Hence the efficacy of a current of air in increasing the dissipating power of the lamp.

When the latter is not placed upon the electrified body, but abstracts electricity from it at a distance, the effect is still produced in a similar way. The electricity on the conductor induces an opposite electricity in the lamp, which, ascending to the flame, is radiated into the surrounding atmosphere, electrifies its particles, and these last, attracted to the charged body, gradually neutralize its electricity. Hence if there is not a free communication through the atmosphere between the lamp and the conductor, the latter will not be deprived of its electricity; but the radiation of induced electricity, from the lamp, will still go on, until the accumulation on the obstacle neutralizes the action of the charged surface. Admitting the truth of this hypothesis, it seems that the abstracting should decrease faster than the fourth power of the distance; for the effect of induction, or the propelling power, we know to decrease as the square of the distance; and the intensity of the electricity when propelled, were there no resistance from the air, would decrease in the same ratio. I have examined the diminution attentively, and find it to agree with the fourth power of the distance within a certain limit, beyond which it appears to decrease faster. But these experiments require extreme caution when much accuracy is required, and are subject, after remaining regular and consistent for many repetitions, to fluctuations that appear at first altogether arbitrary, but which I have since been led to ascribe to the capacity of the atmosphere for electricity; though it must be confessed that if this be the cause, and I have diligently endeavoured to avoid every other, the electrical state of the atmosphere must vary to an extent and with a rapidity which I am not inclined to admit without further experiments *.

---

* All the electrometers with which I am acquainted, not excepting Coulomb's, are imperfect when electricity of much intensity is to be measured; both from their insulating supports taking a charge, and from the glass which is placed over them becoming charged by radiation. Coulomb has in some measure avoided the first by measuring only a small quantity of the electricity to be examined, and the second defect he has likewise attempted to remove by using a very large case; but though his balance is delicate, theoretically elegant, and often the best that can be used in practice, it is cumbrous from its size, tedious in its operation, and only lessens to a certain extent the evils of which I have spoken. For experiments which required less time, and where portability was a convenience, I have made use of the electrometer delineated in *Fig.* 6. A B

Fig. 6.



is a delicate magnetic needle two or three inches long, having its ends furnished with small pith balls. M and N are large balls of thin brass attached to the extremities of a wire M *m* *n* N, which is fixed to the metal foot of the instrument. The electrometer can be placed on the

This hypothesis agrees with the experiments already detailed, but it remains for us to inquire concerning the mode in which flame impresses on electricity a velocity of radiation: which will connect our subject with the laws that regulate the equilibrium of electricity when developed on conducting surfaces. CAVENDISH established these laws nearly as they are now received, but his mathematical acquirements were scarcely adequate to encounter a subject of such extraordinary difficulty, whilst by adopting the hypothesis of FRANKLIN, he was led to a theory less perspicuous and simple than would have resulted from that of DUFAY. POISSON aided by LAPLACE, and conversant with the most refined inventions of the modern analysis, has advanced further in this intricate subject, and has shewn the exact coincidence of theory and experiment. The same coincidence results from either hypothesis, but not with equal simplicity and clearness. The advocates for a single fluid have relied chiefly on the direction of the spark; asserting that it always moves from positive to negative. Were not this assertion supported by names of the first respectability, and nearly universally admitted, I should not hesitate to say that it is utterly without foundation; but against such a weight of testimony even the evidence of the senses should be received with caution and distrust. From my own experiments I am led to believe that by varying the mode in which sparks are drawn from the prime conductor, they may be made to move in either direction. If we present a ball to the extremity of the prime conductor where the electricity is strong, and to the

---

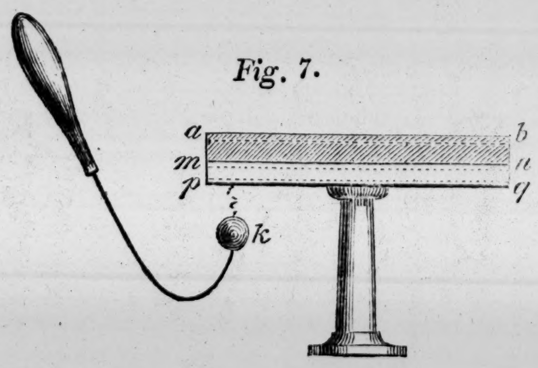
insulated body, or screwed to it, and after being adjusted to the magnetic meridian, and the electricity communicated, the deviations are measured by the sight K, which traverses on an arc that is independent of the instrument. The support of the balance should be as slender as possible compared with the surface of the body electrified, otherwise the quantity of the fluid which is at first rendered inactive on the surface of the support, and which afterwards returns as the electricity becomes faint, interferes materially with the indications of the instrument.

Other, and very serious difficulties present themselves in the experiments which I have described: the least motion of the air alters the rate of abstraction, whilst the radiation of electricity from the light would rapidly charge the sides of any cover of moderate dimensions, and by saturating the inclosed air with electricity would affect the velocity of radiation. These difficulties have hitherto prevented me from determining whether any difference exists in the radiation of positive and negative electricity.

centre, where it is feeble, the direction of the spark will generally vary. The expelling force, as LAPLACE has shewn, is proportional to the thickness of the shell of fluid at the place where the spark takes place; hence to produce a spark from negative to positive, we must choose a part of the positive conductor where the electricity is weak, but where the action of the whole conductor would be much increased at an inch or two above the surface. In a cylindrical conductor, this may generally be effected by making a ball project some distance from the middle of the conductor, and bringing the negative ball, which should be connected by a chain with the negative conductor, over the former.

But it occurred to me that a much more decisive experiment might be made by disposing the apparatus in such a manner that the negative fluid should be strongly impelled to quit the surface, whilst a body at a short distance from the surface should be slightly acted on. The phenomena of charged plates will readily suggest such a disposition. I have before mentioned that a plate intensely charged will not affect a gold leaf electrometer on the neutralized side; the action of the two surfaces neutralizing each other at nearly all distances—whilst on the charged side the action is incomparably less than that by which the two fluids attract each other. If therefore we can obtain such an arrangement of plates, that instead of an attraction for the negative fluid there shall be a repulsion, we may expect to have the spark from negative to positive. Such n arrangement is found in the electrophorus. When this instrument is placed on its insulated stand, and the cover lifted, the electricity distributes itself as follows. The upper part, *a b*, of the resin, *fig.* 7, will be negative, the upper sur-

Fig. 7.



face, *m n*, of the sole will be positive, and the lower surface *p q* will be negative. The quantity of fluid *p q* is nearly equal to that at *a b*, and consequently the force with which it endeavours to escape, and which we have before remarked is proportional to the thickness of the stratum of fluid, will be considerable. But at the distance of the ball *k*, the attraction of the plate *p q* will be nearly neutralized by that of *m n*; and we shall have a much greater thickness of stratum at *p q* than on the ball, and accordingly the spark will be found to move *to* the ball. That the fluid at the lower surface of the sole is negative may be shewn by removing a small part of it on a proof plate, and testing it by a gold leaf electrometer.

It has also been urged by those who suppose the electric fluid to circulate from positive to negative, that when a piece of tin-foil is placed on *Lane's* discharger, the burr always appears on the negative side. I have frequently made the experiment, and when the foil lay loosely on the negative ball, have always found the reverse to be true. Even the different appearance of the light thrown off by a pointed wire when positively or negatively electrified may be imitated with one fluid by arming the wire alternately with a ball of pith gilt and not gilt; the brush of rays seems produced by a resistance experienced in leaving the pith ball.

Supposing the existence of two fluids established, and it should be observed that *one* clear instance of the spark moving from negative to positive is in this case equivalent to such a proof, there can be no objection to admit Poisson's theory, which supposes either fluid, when accumulated in a conductor, to move by the mutual repulsion of its particles to the surface of the body, and to be retained there by the pressure of the air; which Poisson regards as a vase encompassing the electric fluid. But if the air is the immediate cause that retains the electricity, it can only do so by repulsion; whereas all the phenomena of radiation imply an attraction between either of the electric fluids and the particles of atmospheric air. We must therefore conceive the particles of the electric fluid on their first arrival at the surface of a conductor not to be retained there, but to pass on into the surrounding atmosphere; where, attracted by the molecules of air, they quickly form an electric atmosphere of the same kind as the electricity in the conductor; the repulsion of these electrified molecules will, after a time, equal the expanding power of the fluid in the latter, and will then prevent the further escape of the electricity. We shall examine this hypothesis further presently; but we may remark, that the atmosphere of electricity which is supposed to surround all conducting bodies, must be extremely limited, as is sufficiently evinced by an experiment of Franklin, who electrified a small ball, and after repeatedly whirling it round his head by a silk string, found the ball still electrified. The experiment may be readily repeated with the small plate we used at the commencement of these experiments, and which, when tested by the gold leaf electrometer, shews scarcely any loss of electricity after being swung round many times.

So far we have a clear idea of the process by which electricity is retained on the surface of conductors; and if we admit that the apparent conducting power of a vacuum is, in fact, a radiation, we shall be enabled to trace the successive steps which take place in the action of flame upon electricity. POISSON and BIOT seem to consider the fact, that a perfect vacuum is a conductor as settled; though the well-known experiments of MORGAN are opposed to this conclusion *; but these experiments are inconsistent with nearly all others that have been made on the subject, especially with those detailed by CAVALLO. Indeed, it is difficult to conclude anything decided from MORGAN'S experiments; for if the hypothesis which he suggested, and which BIOT has rendered highly probable, be true, that the light observed in electrical experiments is produced by the compression of the air, light ought not to have been seen in a perfect vacuum. And with respect to the charging of the tube, it is to be observed, that the electricity would not flow so rapidly through a vacuum, where it acts by radiation, as it would through a conductor, where it acts by induction, and where the charge on one surface empties itself at once upon the other. Unless, therefore, MORGAN drew off the charge into a condenser, I do not know how he was to prove that the tube was not charged. He does not inform us on

---

* Treatise on Electricity, vol. ii. p. 90.

what he grounded this conclusion, unless on some vague and imperfectly described facts respecting the breaking of the tubes.

Since electricity radiates in a vacuum, part of the radiation produced at the temperature of combustion may arise from the dilation of the surrounding air by the heat; but this cause does not appear sufficient to produce the whole of the velocity impressed. At the highest temperature which we can assign for the combustion of oil, the contiguous atmosphere cannot be more than four or five times less dense than in its natural state; and although electricity of any intensity would radiate freely in such an atmosphere, we cannot imagine the very weak electricity which would be induced at two feet distance by a ball of an inch and a half diameter, moderately charged, to radiate under such a pressure, at least with sufficient force to penetrate two feet of air. We must, therefore, either conclude that a force of radiation is impressed by the heat on the electric fluid, and that this latter, aided by the rare state of the immediately surrounding medium, penetrates it to some depth; or, that flame is of itself a perfect conductor, and that the electric fluid, in expanding through it, obtains from the continued repulsion of its particles the requisite velocity. In either case the surrounding atmosphere would speedily become charged with electricity, whose repulsion would prevent the radiation from going on. Of this effect we have abundant proof; a lamp placed on an electrified conductor reduces its charge almost instantaneously to a certain strength, after which it acts very slowly, unless aided by a current of air. Thus an electrified body, when heated to the temperature of combustion, differs from other conductors merely in being surrounded by a much more extended atmosphere of electricity. The extent, in the former case, apparently amounting to several feet, whilst in the latter, it cannot exceed a very minute fraction of an inch, as we see by the experiment of FRANKLIN already adduced; and as is more decidedly shewn by the effect of points in discharging electricity. It seems commonly supposed, that a very sharp point will discharge an electrified body immediately, and whilst we confine ourselves to strong charges the fact is true; but if an insulated conductor be furnished with a delicate electrometer, it will be found to retain a small charge for nearly an hour, although a fine sewing needle projects an inch or two from its surface; nor will the action of the latter be much accelerated, though the charged air be removed from around it by a pair of bellows. The action of the flame and the point are different; the former never accumulates electricity enough to overcome the pressure of the atmosphere, but radiates the fluid either to or between the molecules of air; the latter does accumulate electricity enough to push back the molecules immediately opposed to its point, and the surrounding air rushing into the minute vacuum thus formed, creates that current which is always felt to proceed from such a point. As the electricity gets weaker, this vacuum contracts, and no points being perfectly sharp, there will be an intensity of electricity, which is balanced by the pressure of the air even when accumulated at the point of the finest needle. The action of a point depends chiefly on this vacuum, through which the electricity radiates, as through a crack in the vase that surrounds it: but the lamp is surrounded by an atmosphere of electrified particles of air, and its action will be impeded and stopped, unless these are removed; when abstracting electricity from a distant body, the effect of a current towards this last will be still more powerful, since it then forms a medium of communication between the two, and this circumstance forms one of the greatest difficulties which we have to encounter in such experiments.

I shall conclude this part of the subject with some remarks upon the analogous cases which occur in phenomena connected with electricity. It has been long remarked, that all bodies when nearly red-hot become good conductors; those which soften before this temperature become so from their change of state; but glass, a very perfect non-conductor, becomes a conductor at a lower temperature than that at which it softens, and the same is probably true of other bodies: if such is the case, we must attribute the effect either to bodies at that temperature losing their attraction for electricity, or, which is more probable, to the radiating force impressed by the heat. The magnetic experiments which I before mentioned are still more striking; the magnetic fluid, whether in the form which we meet it in the loadstone, or under that modification which forms the electro-magnetic fluid, is possessed of so little mobility, or meets such obstruction in flowing through bodies, that there are very few that will serve as conductors to it. Iron was long supposed to be the only material in which the magnetic fluid was developed; and the electro-magnetic fluid is so feeble in its motive powers, that when generated by a battery of a few large plates, it cannot be made to flow through water, and a single thickness of silk is sufficient to arrest its progress. Yet when bars of iron are heated white-hot, the magnetism induced by the earth seems to pervade it with such rapidity, that it ceases to act on the compass. It is true, that we know very little of the real cause of this phenomenon; but when we find exactly the same temperature to produce the radiation of electricity, and of the intermediate galvanic fluid, it seems probable that the effects must be the result of similar causes. The radiation of the galvanic fluid may also be doubted, but the circumstances are so precisely similar to those which occur in electricity, that I consider them as quite satisfactory. If the wires of a battery of large surface be furnished with charcoal points, and brought within the tenth of an inch asunder, no effect is produced; but when they are separated by an interval which does not exceed one fiftieth of an inch, a bright spark appears, the charcoal points become intensely ignited, and a stream of fire commences flowing from one point to the other. The points may now be separated many inches asunder, and the fluid will continue to flow in a stream of the brightest light. From the positive point it parts in a stream resembling that which issues from a pointed wire on a conductor electrified positively, whilst the negative fluid presents that appearance of a star, so well known as characteristic of electricity flowing into a negative conductor. These phenomena are precisely such as we might expect: whilst the charcoal is not ignited, the galvanic fluid is incapable of penetrating a plate of air more than the fiftieth of an inch in thickness. Brought within this limit, it overcomes the obstacle opposed to its passage, and the immense quantity of fluid which then begins to circulate, raises the temperature of the charcoal till it ignites, and the fluid obtains from this elevation of temperature, a power of radiation which suffices to penetrate a plate of air several inches thick*. If this view of the subject be correct, the phenomena should take place more readily when the charcoal is previously ignited. I have not made any satisfactory experiments upon this subject. The fact that light is thrown off abundantly at the radiating temperature is a singular coincidence, and when united with the known fact that few of the solar phosphori act till they are warmed, and that some of them which give out blue or yellow light when nearly cold, give off the red rays when heated, would seem to point out an analogy between the radiation of light and that of electricity.

---

* The effect ought, theoretically, to be increased in a vacuum; and we are assured by Biot, that such is the case. Precis. I. 648.

With respect to the latter, the preceding pages contain, I believe, the first inquiry into the subject; and much must be done before we can consider the effect of caloric upon electricity as fully developed. My own views will be answered, if I succeed in attracting the attention of electricians to the subject, which promises to afford a favourable method of inquiring into the laws which regulate the motions of the electric fluids, a branch of the science far more imperfect than that which treats of their equilibrium. In concluding, I shall make a few remarks upon this last, as it is exhibited in the works of Poisson and Biot; the former of whom has treated the subject more profoundly, and the latter more methodically, than any preceding writers.

These philosophers set out from the law which Coulomb demonstrates, that the particles of the electric fluids repel each other with a force that varies inversely as the square of the distance; and assuming that conducting bodies have no action on electricity, they conclude that the latter will be spread over their surface only, where, if the body be a sphere, it will form a shell of uniform thickness; pressed on its external surface by the atmosphere which prevents the dissipation of the fluid.

But this conclusion involves several hypotheses: in the first place it is necessary that the law of repulsion between the particles of the electric fluid should differ from the inverse square of the distance when the particles are brought very near together; for were that law to hold, neither would a spherical shell of fluid be in equilibrium, nor would it be retained by the pressure of the air. With such an arrangement, the interior particles of the shell could not be acted on, and consequently they would continue to approach those which were without them until they actually touched; and, strange as this result may appear, it was adopted by Cavendish in his theory. Biot, in the *Traite de Physique,* has not remarked this difficulty, and consequently, the explanation which he has given after Poisson, of the equilibrium of the electric fluids, is not sufficient. In a subsequent work, however, he has observed, that the absence of electricity in the interior of bodies leads to the important result, that the electric fluid is incompressible, and in other instances speaks of it as flowing after the manner of liquids; such comparisons are probably merely inaccurate modes of expression. If the phenomena of electricity be explained by the intervention of peculiar fluids, these must be assumed as elastic, but when compressed within certain limits, increasing the law of their repulsion to a high power of the inverse distance. The chief difference between fluids so constituted, and such elastic fluids as the gases, consists in the latter diffusing themselves uniformly, whilst the former everywhere press to their boundaries, and if not free, almost immediately acquire a uniform velocity of radiation.

A second hypothesis, which is required to explain the equilibrium of the electric fluids at the surface of conductors is, that the atmosphere immediately surrounding the conductor, becomes highly charged with the same species of electricity, which, by its repulsion, retains that on the surface: for the air of itself cannot retain it there, having no repulsion, but rather an attraction for each of the electricities; as is shewn by many experiments: it is impossible to work an electric machine for any length of time without charging the air of the apartment with electricity; if a screen of tin foil connected with an electrometer (*fig.*4), be carried into the room, it will immediately render sensible the electricity which is diffused there, and which must be adhering to the particles of air, and forming minute atmospheres of electricity round them. Thus, when a conductor is electrified, the electricity radiates to the surface, and a small portion of it passing on, attaches itself to the immediately contiguous particles of air, where it is retained by the attraction of the latter, and by its own repulsion prevents the further escape of the fluid. The quantity so accumulated in the adjoining particles of air need be very small to preserve the equilibrium. If we assume the particles of air and of the electric fluids to be at the same distance under equal pressures, it would be sufficient for one particle of the former to attach itself to one of the latter; hence there is no proof from the experiment of Franklin before mentioned, that such electrified atmospheres do not exist.

The thickness of the shell of fluid which can be retained by a given pressure, affords an excellent means of comparing different electrometers, or rather furnishes a standard by which to compare them. If t is the thickness of the shell, m a constant co-efficient, and h the height of the barometer, we have

$$t = m\sqrt{h}$$

or, assuming $t = 1$ when $h = 30$ inches

$$t = 182\sqrt{h}$$

An electric machine in good order will readily charge a conductor high enough for the thickness of the shell of fluid on a ball an inch in diameter, and projecting some inches from the conductor, to exceed this limit, and consequently to continue discharging itself into the air.

With the suppositions which I have mentioned, and which there appears every reason to believe correct, the theory of the equilibrium of electricity as improved by Poisson from the prior theories of Æpinus and Cavendish, leaves little to be wished for on this part of the subject, and may readily be made to embrace both the equilibrium of the magnetic fluids and that of the electricity disturbed by the action of the electromotive apparatus. The first of these applications I made in 1820, in a couple of papers published in the 55th volume of the "Philosophical Magazine;" where, proceeding upon the theory which Poisson had applied with so much success to the distribution of electricity on spheres and ellipsoids, I deduced, by a very easy investigation, the distribution of the magnetic fluids, on similar solids when subject to the action of the earth. The result, which was wholly theoretic, agreed with all the experiments of Mr. Barlow, and seemed to leave no doubt respecting the truth of the principles. Examining more nearly however, we found an error of one-sixteenth in the constant co-efficient by which the several expressions for the deviation were multiplied. This minute error arises from the resistance which iron offers to the motion of the magnetic fluids; Mr. Barlow, however, gave it another explanation, and some phenomena, which do not admit of a ready mathematical calculation, seemed to favour the result. I mention this circumstance in consequence of some remarks of an anonymous critic who speaks slightingly of the calculations in Mr. Barlow's work, and gives great praise to a subsequent paper of Poisson.—The results and theory of this paper are precisely those which I had published three years before; the method of investigation is the same as that before employed for determining the equilibrium of electricity; and though not admitting of any definite results, is profound and subtle in the extreme. The question in its greatest generality belongs to a class of problems which have hitherto baffled the powers of the greatest mathematicians of Europe: the condition of equilibrium on bodies of any form is expressed by a simple equation in definite integrals, or, as such cases might be more properly termed, limited functions, and notwithstanding the talent with which this highly important subject has been treated, it yet remains unexplored, a rich field for some future LAPLACE.

I am not aware whether any one has remarked that the distribution of the electric fluid in Volta's pile, when the ends are isolated, is, with respect to the service, the same as that of the magnetic fluids in a magnet induced by the earth. The constant disturbing force at each surface of copper and zinc must be overcome by the action of the fluids already developed, as the constant action of the earth, minus the constant resistance to motion, has to be overcome in the magnet. Hence a spheroidal pile generated by the revolution of an ellipse will have for the boundary of the shell of fluid, a similar and equal ellipse whose centre is at a very small distance from that of the pile. The quantity of fluid developed will be known immediately we know the motive force of each surface, and if the latter were equal in intensity to the magnetic attraction of the earth, the electrical attraction and repulsion of the extremities of the pile would be equal to the similar magnetic actions of a mass of soft iron of the same shape and size as the pile, and placed in the direction of the dip. It is still a question whether the electric fluid which accumulates in an isolated pile, and that which circulates in one that is closed, are identical;

and one of the difficulties met with in investigating the subject arises from the low intensity of the electricity in the former. This may certainly be greatly increased by making the pile in the form of a double cone, having the ends completely isolated; but such a construction is attended with some practical difficulty. How far it would augment the energy of the current when closed, can scarcely be conjectured in the present state of our knowledge respecting this species of action.

On the French Claim to the Discovery of the Steam-Engine.

[In a Letter to the Editor.]

Sir,—I observe in the French *Constitutionnel,* of last week, that Stuart's work on steam-engines is quoted as an honourable confession, that the steam-engine originated in a Frenchman, and is attributed to *Salomon de Caus,* of Dauphiny,—and being possessed of that rather scarce work, his *Raisons de Forces Mouvantes,* it will be not uninteresting to shew the nature of his claims. I therefore subjoin the title and extracts, together with a copy of his figure of the machine he employed; and as it may be amusing to your readers, add the contents of the two other books which accompany it.

"*Les Raisons des Forces Mouvantes, avec diverses Machines, &c. Par Salomon de Caus, Ingénieur de Son Altesse Palatine Electoriale.—A Francfort,* 1615.

"The work is a thin folio with many plates, and is divided into three books—the first of which treats of mechanical powers, with 35 problems and 44 plates, commencing with his definitions of the Elements: in the first theorem he gives a wooden cut of a globe of copper, which has a tube introduced at the side; this descends nearly to the bottom, and has a stop-cock on the end which issues from the side; on the summit is another aperture, closed with a stopper, or valve; when this has been filled with one-third of water, a fire is to be put under it for three or four minutes, and then the side-cock turned, when by the force of the steam, the whole of the water, he adds, will be discharged, and the air remaining must be discharged by the upper valve or stop-cock, when a portion of the water will be found to have been evaporated.

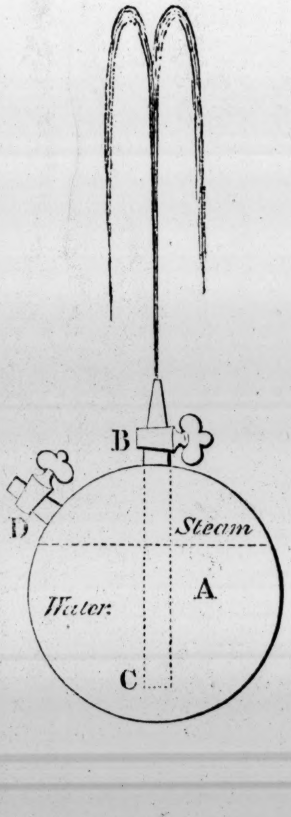
"Fill the vessel then," says he, "again as before, one-third, closing well both the tube and upper hole, and replacing the fire under for the same space of time—take the vessel off the fire and let it cool, without opening either of the apertures; and on discharging the water it will be found that the quantity is not diminished, since the vapour has been condensed in this experiment.

"We may give a third demonstration of this—which is, that when having replaced the same quantity of water in the vessel, we stop well the stop-cock of the bent tube, which descends to the bottom of the vessel, and open the upper stopper, when placing the vessel again on the fire, and placing a jug below it, the water will boil by the heat, and discharge itself by the stop-cock of the bent tube, but a fifth or sixth part of the water will remain, on account of the violence of the *steam which causes the water to rise*, and hinders the whole from being discharged —which steam afterwards issues with great violence.—He adds afterwards, that he had tried the same experiment with quicksilver.—Here, therefore, we have the steam-boiler in miniature, and its effects—but in his fifth theorem, which he entitles,

"*Water raised by fire above its level.*

"He uses the same boiler in another form, passing the tube from the upper part of the ball of copper, downwards, towards the bottom, instead of passing it by a curve from the side, and thus describes his method.

"The third means of making water ascend *is by the aid of fire*, in which divers machines may be employed, one of which I here annex.—Make a ball of copper, A, well soldered up, to which there is a cock to introduce the water by; and a pipe marked B C soldered on to the ball, the end of which at C approaches the bottom without touching it: having injected the



water by the stop-cock, close it well, and set the vessel on the fire, when the heat given to the ball will cause the water to ascend by the pipe B C."

The second part, as well as the first, has handsomely engraved folio frontispieces, ornamented with figures in the style of the times, and contains aviaries, grottoes, and fountains, in twenty plates, containing the machinery used to exhibit them, many of which are very trifling, and are dedicated to *Elizabeth*, Princess of Great Britain, and Electrice Palatine, &c.

The third book is wholly employed in describing the fabrication of organs, such as casting the pipes, &c., two plates, with several wood-cuts. You see, therefore, that the principle of raising water by steam may, for anything I know, have been first suggested by the French engineer; but that he had any idea of the modern steam-engine must be denied; or that he suggested the machine proposed by the Marquis of Worcester, to raise water for the city of Westminster. I therefore think you will do well to publish it in your next Number, lest, from the manner in which this claim for France is brought forward, some persons might be induced to give credit to the assertion.

I am, Sir, yours, &c.
G. CUMBERLAND.

Bristol, March 2, 1829.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

i. *Elementary View of the* UNDULATORY *Theory of Light.*

By Mr. FRESNEL.

[Concluded from the Number for January.]

Modification of Polarised Light by Reflection.

I HAVE found that the total reflection of light in the interior of glass, at an inclination of about 50° from the perpendicular, produced this peculiar modification in the light, when it had first been polarised at an azimuth of 45°, reckoning from the plane of reflection: that is to say, the reflected light was then composed of two equal systems of undulations, polarised at right angles to each other, and differing one-fourth of an undulation in their oscillations. This reflected light, which presents no further trace of polarisation, when it is analysed with a rhomboid of calcarious spar, has the property, like polarised light, of exhibiting very bright colours in crystallised thin plates: but the colours are of an uncommon nature. It differs also from polarised light in not sensibly developing colours in oil of turpentine, and in plates of rock crystal perpendicular to the axis. When it is subjected again to two total reflections at the same incidence and in the same plane, or in a perpendicular plane, it resumes all the characters and properties of common polarised light: when it is subjected to two more reflections of the same kind in the same directions, it is completely depolarised, and recovers at the same time the other properties which it had derived from the first two reflections, and so forth.

I shall not enter into more particular details of this singular modification of light, which is impressed at once on all the different kinds of rays like polarisation itself; and in this respects exhibits properties equally general. I shall only observe, that it was the nature of the tints developed by light, so modified in crystallised plates, that pointed out to me, that it must be composed of two systems of undulations, polarised at right angles, and differing a quarter of an undulation in their progress; and setting out from this fact, I had no difficulty in explaining and in calculating the various phenomena which it exhibits, with the assistance of the same principles which have enabled us to calculate the tints produced by common polarised light.

Before I discovered these modifications, produced in polarised light by total reflection, I had studied those which are produced by partial reflection on the external surface of transparent bodies, and I had observed that the light is in this case never depolarised, even in part, and that whatever may be the inclination and the relative situation of the plane of incidence, it produces no change but a simple alteration of the plane of polarisation. The new hypothesis that I have adopted, respecting the constitution of the luminous undulations, has pointed out to me the law of these deviations, which I had before in vain attempted to represent by empirical formulas. These formulas agreed well enough with the phenomena in the three principal cases, of rays parallel to the surface, a perpendicular incidence, and a complete polarisation: but they did not agree correctly with the facts at intermediate incidences. The formula to which I have at last been led by theoretical considerations, and which will be found in an addition to the note which I have already mentioned, (*Ann. Chim.* xvii. p. 312,) seems to express correctly the law of the phenomenon, if we may judge by its agreement with observations. I have deduced it from the general formulas for the intensity of reflected light, which I have derived from the same considerations, and which I have also published in the same note.

I shall here terminate this extract from my memoirs, and I shall not enter into the theoretical and experimental researches which I have undertaken, in examining the phenomena of polarisation, discovered by M. Biot in certain homogeneous liquids, such as the essence of turpentine, and the essence of lemon. I have thought it necessary to confine myself to the explanation of the more general properties of light, and to the elementary facts, if I may so call them, or those which occur the most frequently, and of which others are in some sense only more or less complicated combinations. I have shown in what manner the theory of undulations might be employed for explaining them, and for furnishing the means of representing, by analytical expressions, the laws which govern them. In order to calculate the phenomena of diffraction, which are so diversified, those of the coloured rings produced by a thin plate of air or of any other refractive medium; the law of refraction itself, in which the proportion of the sines of incidence and refraction is exactly that of the velocities in the two mediums; the colours and the singular modes of polarisation, which are obtained from crystallized plates, it is sufficient to know the different lengths of the undulations of light, in the mediums which transmit it, and this is the only quantity that we are obliged to borrow from experiment, as the basis of all our formulas. If we attend to the intimate and multiplied relations which the theory of undulations establishes between the most different phenomena, we must be struck at once with its simplicity and its fecundity; and we must allow, that even if it had not the advantage over the system of emanation, that it explains many facts wholly unaccountable in this latter system, it would amply deserve a preference from the means which it affords us of connecting together all the phenomena of optics in its general formulas.

Without doubt there are still many obscurities to be enlightened, especially such as relate to the absorption of light, for instance, in the reflection of metallic surfaces, and in black bodies; the passage of light through bodies imperfectly transparent, and the proper colours of bodies. It is probable, that in these different cases, a part of the light changes its nature, and affords calorific vibrations, which are no longer sensible to our eyes, because they cannot penetrate their substance, or cannot make the optic nerve vibrate in unison with them, on account of the modifications which they have undergone. But the total quantity of the living force must remain the same, unless the action of light have produced a chemical or a calorific effect, sufficiently powerful to change the state of equilibrium of the particles of the bodies, and with it the identity of the forces to which they are subjected: for it is easily understood, that if these forces were suddenly weakened, there would be a sudden diminution of the energy of the oscillations in the particles of the heated body, and consequently an absorption of heat, to use the common expression. And this is perhaps what happens in the melting of a solid, or in the vaporisation of a liquid.

If light is only a certain mode of vibration in a universal fluid, as the phenomena of diffraction show, we must no longer suppose that its chemical action on material substances consists in a combination of its particles with theirs; but in a mechanical action, which the particles of this fluid exercise on the ponderable particles, and which causes them to enter into new arrangements, into more stable forms of equilibrium, from the peculiar kind or energy of the vibrations to which they are exposed. It is obvious, that the hypothesis adopted respecting the nature of light and heat, may materially influence our conception of the nature of chemical actions, and that it is very important not to be deceived in our fundamental notions of this theory, in order that we may hope to arrive at last at the discovery of the principles of atomic mechanics, the knowledge of which would throw so much light on the whole of chemistry. If any thing can contribute very essentially to advance this great discovery, and to assist us in penetrating the secrets of the internal constitution of bodies, it must be the minute and indefatigable study of the phenomena of light.

POSTSCRIPT.—*On the Chemical Action of Light.*

Mr. ARAGO has lately confirmed, by a very interesting experiment, the opinion of Mr. FRESNEL, respecting the chemical action of light, and has demonstrated that it cannot be attributed to the combination of its particles with those of material bodies.

Experiment.

When the fringes produced by the interference of two pencils, reflected by mirrors slightly inclined to each other, were thrown on some muriate of silver, recently prepared, Mr. ARAGO has found that they marked on it black lines, placed at equal intervals, and separated by white spaces: and this shows that the chemical influence of the rays of light is modified by their interference, as well as their optical properties, and that it varies in intensity, according to the difference of the paths described by them. When this difference is equal to a whole number of undulations, the two systems of undulations are in perfect agreement, and their oscillations have the greatest possible energy; it is then that their chemical effects become a maximum; when the difference of the paths, on the contrary, is an uneven number of undulations, the discordance being complete, the chemical effects ought to disappear, as well as the sensation of light arising from the same points in the eye: and this is equally confirmed by experiment. It must only be remarked, that the extreme violet rays being those which have the greatest chemical action, the black lines traced on the muriate of silver ought not to correspond to the brightest parts of the fringes produced by white light, which answer nearly to the points of perfect coincidence for the yellow rays: and this experiment affords a simple and very accurate mode of determining the mean length of the luminous undulations which have the most chemical influence; for it is sufficient for this purpose to measure the intervals between the middle points of the black lines marked on the muriate; and the formula given in this essay will determine the length of the undulations which produce them.

Dr. Young had long ago shown, by throwing light modified by the coloured rings, on the muriate of silver, that the same modifications affected its chemical action: but the experiment of M. Arago has the further "advantage" of proving directly, that the unequal action of the light on the different points in which the two pencils meet, depends on their mutual influence; because, when we intercept either of the pencils, we see the muriate of silver assume a uniform tint in the same space which was marked by black and white lines, when both the pencils fell on it; while in Dr. Young's experiment, made with the coloured rings, it was impossible to separate the two systems of undulations. It is also demonstrable by the experiment of Mr. Arago, that in all the points which answer to differences of paths, equal to an odd number of semi-undulations, the chemical action of the light is insensible, when the two pencils arrive there together, and that it reappears when one of the pencils is withdrawn. This fact alone, *independently of all theory*, reverses the supposition adopted by several philosophers, according to which the chemical effects of light depend on its combination with material substances; for if this were the case, there would always be a greater effect when the number of luminous particles become more considerable, and it would be impossible to augment the chemical action of light, by withdrawing a part of the incident rays.

Mr. ARAGO's experiment contains also another remarkable circumstance, which does not exist in that of Dr. YOUNG; in this latter, the rays are parallel, and do not again separate after their combination; while the rays reflected by the mirrors form a sensible angle, and allow us to observe, that having once lost their luminous and chemical properties, by their complete discordance with each other, they recover them again under other circumstances a little further off; which shows, as Mr. ARAGO observes, that they were not actually destroyed, but only momentarily neutralised, as long as there were motions in opposite directions to counteract their oscillations. This restoration may easily be understood by an inspection of the first diagram in this abstract.

Mr. ARAGO's experiment requires several precautions in order to be repeated with success. The solar rays, thrown into the dark chamber, must be kept in a constant direction by a good heliostate, in order that the fringes thrown on the surface of the muriate should undergo no sensible change of place, for ten minutes at least: and in order that the very small changes, which are unavoidable, should not injure the neatness of the black lines which they form, it is proper to give the fringes the greatest possible breadth, by placing the mirrors very nearly in the same plane: and instead of a common lens in the window shutter, to form a luminous point, which would afford by much too feeble a light, it is necessary to employ a cylindrical lens, which gives us a very valuable command of the intensity of light; but since this affords a linear image instead of a point, we must be very careful to turn it into a direction perfectly parallel to the fringes: and the distinctness of the fringes will serve as a sufficiently correct test of the performance of this condition. The cylindrical lens, which was employed, was about four tenths of an inch in focal length; the two metallic mirrors were at the distance of about two feet from it, and the plate, covered with the muriate of silver, was about the same distance from the mirrors; and it was necessary that the distances should be no greater, in order to preserve a sufficient intensity in the light. It must be remarked, that if the fringes had been very fine, they would have become confused under these circumstances, because of the sensible breadth of the luminous line afforded by a lens of the magnitude described: and it is for this reason that it is necessary to make the fringes as broad as possible. A finer line might be obtained with a lens of shorter focus, but it would be necessary to alter the other dimensions of the apparatus in the same proportion, and the fringes could not be made broad enough to diminish the confusion. This is the most difficult part of the experiment to manage; but with a little address, and a great deal of patience, it may always be accomplished.

[Dr. YOUNG's experiment was made with a solar microscope, employing the common apparatus for viewing opaque objects; and there is really no great difficulty in conducting it, the common attention for keeping the sun's light in the field being all that is necessary to keep up the illumination, which can only vary in its intensity, the place of each point of the rings being perfectly fixed. Dr. YOUNG did not either perform the experiment with great care, or even once repeat it; but he satisfied himself, that the rings of the darkening rays were a little *smaller* than the rings of violet light, and that they were *not much* smaller. Mr. FRESNEL has not told us how far this conclusion was confirmed by the " much later," experiment of Mr. ARAGO.]

---

ii. *Rules for finding the* DIFFERENCE *of* LONGITUDE *and the* ELLIPTICITY *of the Earth, from the Latitudes and relative Azimuths of* TWO STATIONS.

A. MULTIPLY the cosecant of each of the latitudes by the cotangent of half the sum of the observed azimuths; the products will be the tangents of two angles, of which the sum is the difference of *longitude*.

B. Divide the difference of longitude by the difference of latitude, and multiply the quotient by the tangent of half the difference of the observed azimuths, and again by the square of the cosine of the mean latitude: subtract the product from unity, and multiply the remainder by the square of the secant of the mean latitude: one fourth of the product will be the *ellipticity*, as deduced from the two *curvatures* of the zone between the stations.

C. Multiply together the cosecant of the first azimuth, the sine of the second, the secant of the first latitude, and the cosine of the second: the square of the product, lessened by unity, will be the numerator, and the same square, multiplied by the square of the sine of the first latitude, and diminished by the square of the sine of the second, will be the denominator of a fraction denoting twice the *ellipticity*, as deduced from the general *inclination* of the zone.

EXAMPLE.

Latitude of Crowborough, from celestial observations, by Captain Kater		51°	3′	18″.30
Latitude of Fairlight		50	52	36.88
Difference	(641″.42)	0	10	41.42
Sum		101	55	55.18
Mean		50	57	57.59
Azimuth at Crowborough, by the theodolite		121	4	58.36
at Fairlight		58	83	26.14
Sum		179	38	24.50
½ Sum	Compl. 10′ 47″.75	89	49	12.25
Difference		62	31	32.22
½ Difference		31	15	46.11

A.

Log. cosec. 1 Lat.	10.1091596
cot. ½ sum Az.	7.4969836
cosec. 2 Lat.	10.1102546
1 sum L. ta. 13′ 52″.87	7.6061432
2 sum . . 13 54.95	7.6072382
Diff. Long. 27 47.82 = 1667″.82	

B.

Log. diff. Long. . 1667″.82	3.2221492
diff. Lat. . . 641.42	2.8071425
Difference	.4150067
Log. ta. ½ diff. az.	9.7832751
cos. mean lat.	9.7991896
Log. .994195	9.9974714
1 —.005805 Log.	7.7638022
2 Log. sec. mean Lat.	0.4016208
Log. .014636	8.1654230

$\frac{1}{4}$. $.003659 = \frac{1}{273.3}$

C.

Log. cosec. 1 az.		0.0673125
Sin. 2 az.		9.9310315
Sec. 1 lat.		0.2016443
Cos. 2 lat.		9.8000211
Sum		0.0000094
Double, Log.	1.0000433	.0000188
2 Log. sin 1 lat.		9.7816808
Log.	.604922	9.7816996
Sq. sin. 2 lat.	.601853	9.7794908
	.003069	

$\frac{433}{61380} = \frac{1}{142}$

It is not necessary that the results of the rules B and C should be the same, unless the spheroid be truly elliptical: but it is probable that a great part of the difference is here owing to some error of observation: a very small inaccuracy in the latitudes producing a great difference in the ellipticity, as computed by the last rule. They may, however, both serve as useful checks on the whole operation, and when repeated and properly combined, may be employed for determining the figure of the earth with at least sufficient accuracy for the correction of parallax, and other astronomical purposes.

[The DEMONSTRATION, with figures, hereafter.]

---

iii. *Results of Professor* ENCKE's *latest computations respecting the comet "of Pons."*

EMPLOYING the time of the meridian of Paris, and reckoning the right ascensions and declinations from the mean equinox of 1819, for the observations of 1819, and for those of 1822, 1825, and 1829, from the mean equinox of the day of the respective perihelium, that is 1822 May 24, 1825 Sept. 16, and 1829 Jan. 9.7; the places of the comet in the years mentioned, according to direct observations, will be these:

Time.		Right Ascension.	Declination.
1818 Dec.	22.26464	326° 18′ 20″.9	+ 2° 54′ 13″.0
1819 Jan.	1.24963	323 11 45.8	+ 0 14 54.2
	12.23462	315 35 31.9	− 5 35 18.8
1822 June	2.84328	93 46 24.9	+ 16 52 49.0
	12.84643	103 15 15.5	+ 7 6 36.4
	22.86984	115 47 43.0	− 9 10 10.7

Time.	Right Ascension.	Declination.
1825 Aug. 12.57139	100° 54′ 32″.4	+ 31° 32′ 49″.1
17.57139	110 20 15.0	+ 30 15 0.2
22.57139	120 13 22.3	+ 28 4 21.1
27.57139	130 20 27.3	+ 24 56 8.0
Sept. 1.57139	140 29 27.0	+ 20 49 49.2
6.57139	150 35 41.5	+ 15 48 47.4
1828 Oct. 13.30000	7 21 27.5	+ 28 45 8.5
Nov. 6.30000	339 47 19.3	+ 22 57 12.1
25.30000	319 26 53.3	+ 12 48 51.1
Dec. 5.30000	309 53 17.8	+ 6 46 40.4
16.30000	297 59 16.6	− 1 6 22.3

The places for 1819, 1822 and 1825 are deduced from the whole of the observations, and are determined with certainty. Those of 1828 depend on the observations of Berlin only, and may require some modifications, though probably small ones.

Taking for the elements of the orbit,

M, The mean anomaly for a certain epoch.

μ, The mean daily sidereal motion.

ϖ, The longitude of the perihelium.

☊, The longitude of the node.

i, The inclination.

φ, The angle of which the sine is equal to the eccentricity: and for the masses of the planets ☿ $= \frac{1}{2025810}$, ♀ $= \frac{1}{356632}$, ⊖ $= \frac{1}{329630}$, ♂ $= \frac{1}{2546320}$, ♃ $= \frac{1}{1067{\cdot}09}$, ♄ $= \frac{1}{3512}$, neglecting the influence of ♅: the computation of the perturbations, executed with the necessary precision, affords us the following values.

	1819 Jan. 27.25 − 1822 May 24; $t = 1212.75$	1819 Jan. 27.25 − 1825 Sept. 16.3; $t = 2424.05$	1819 Jan 27.25 − 1829 Jan. 9.72; $t = 3635.47$
ΔM,	− 9914″.19	− 184464″.8	− 27284″.62
$\Delta \mu$,	− 7.38993	− 6.68217	− 7.27110
$\Delta \varpi$,	+ 571.78	+ 571.43	+ 606.54
Δ ☊,	− 652.14	− 681.11	− 727.49
Δi,	− 980.53	− 915.95	− 966.26
$\Delta \varphi$,	− 1564.28	− 1400.94	− 1496.78

So that if we denote the elements for 1819 by M_0, μ_0, ϖ_0, ☊$_0$, i_0, ϕ_0, those of any subsequent epoch will be

$$M_0 + \Delta M + \mu t.$$

$$\mu_0 + \Delta \mu.$$

$$\left.\begin{array}{l} \varpi_0 + \Delta \varpi \\ ☊_0 + \Delta ☊ \end{array}\right\} + \text{the precession of the equinoxes.}$$

$$i_0 + \Delta i.$$

$$\varphi_0 + \Delta \varphi.$$

By these values we can never succeed in reconciling the observations of the different years with a single orbit of the comet; and even a change in the masses of the planets is insufficient. If we take the elements which agree the best with the observations of 1819, 1822, and 1825, though with unavoidable discordances of more than two minutes, the return of 1828 will exhibit errors of more than half a degree.

The observations of 1786, 1795, and 1805, had given reason to suspect, before 1819, an augmentation of M proportional to the square of the time: and by endeavouring to apply to this phenomenon the hypothesis of the resistance of an ether, these earlier observations had afforded for the coefficient of this resistance, $U = \frac{1}{752.73}$:

U signifying the force of a resistance which a celestial body of the form and the invariable density of the comet would undergo, if it moved at the distance 1, or the mean distance of the earth, from the sun, with a velocity 1, causing it to describe the distance 1, in a unit of time or a mean day, this force being expressed in parts of the mass of the sun: it having also been assumed that the density of the ether was inversely proportional to the square of the distance from the sun.

Computing from this value, which rests wholly on the observations anterior to 1819, we must add to the perturbations the following quantities.

	1819 Jan. 27 28 – 1822 May 24.0; $t = 1212.75$	1819 Jan, 27,25 – 1825 Sept, 16.3; $t = 2424.05$	1819 Jan. 27.25 – 1829 Jan, 9.72; $t = 3635.47$
ΔM,	$+ 75''.154$	$+ 289''.252$	$+ 644''.934$
$\Delta \mu$,	$+ 0.11937$	$+ 0.23527$	$+ 0.35177$
$\Delta \varphi$,	$- 4.23$	$- 8.37$	$- 12.51$

The elements, derived from this hypothesis, by the observations of 1819, 1822, and 1825, differed from the observations of 1828 from two to five minutes.

By combining in the most advantageous manner possible, without changing the masses of the planets, all the observations of 1819, 1822, 1825 and 1828, we find that the value of U becomes $= \frac{}{857.33}$, being diminished by about $\frac{1}{8}$; and the elements will be

1819 Jan. 27.25

$$M_{\circ} = 359^{\circ}\,59'\,43''.38 + 46''.924 \; \frac{\delta U}{U}$$

$$\mu_{0} = 1076''.80800 - 0''.156664 \; ..$$

$$\varphi_{\circ} = 58^{\circ}\;3'\,20''.4 + 42.038 \; ..$$

$$\varpi_{\circ} = 156\;59\;38.3 + 62.261 \; ..$$

$$☊_{\circ} = 334\;32\;56.1 + 754.311 \; ..$$

$$i_{\circ} = 13\;36\;45.7 + 83.300 \; ..$$

The difference between these elements and the observations are

	A. R.			Decl.		
1819,	+ 44″.2	− 159″.3	$\left(\frac{\delta U}{U}\right)$	+ 47″.8	− 25″.2	$\frac{\delta U}{U}$
	+ 12.6	− 245.4	...	+ 23.8	− 32.9	...
	− 27.9	− 409.2	...	− 6.3	− 83.2	...
1822,	+ 38.5	− 18.4	...	+ 18.5	+ 209.1	...
	− 19.9	+ 13.1	...	− 6.8	+ 220.5	...
	+ 7.9	− 150.6	...	− 10.0	+ 331.7	...
1825,	− 24.4	− 348.5	...	+ 13.2	+ 82.9	...
	− 16.3	− 356.5	...	+ 5.6	+ 125.8	...
	− 5.8	− 353.1	...	− 0.3	+ 170.1	..
	− 4.0	− 341.2	...	− 7.1	+ 212.3	...
	+ 10.2	− 326.9	...	− 9.9	+ 249.5	...
	+ 4.4	− 317.7	...	− 13.1	+ 279.6	...
1828,	− 20.3	+ 113.3	...	+ 20.1	− 70.0	...
	+ 19.0	− 37.6	...	− 6.1	− 26.2	...
	+ 4.6	− 209.3	...	− 19.8	− 25.2	...
	− 1.5	− 299.5	...	− 16.3	− 30.4	...
	− 4.5	− 400.6	...	− 4.6	− 42.1	...

If we put $\frac{\delta U}{U} = -1$, we shall have the elements and the differences without the hypothesis of a resisting medium, determined, so that the sum of the squares of the differences (Δ^2 A. R. $\cos \delta^2 + \Delta\,\delta^2$) may be the least possible. This sum is found, for 34 observations $= 11405 + 1594950 \left(\frac{\delta U}{U}\right)^2$.

The mean error of an observed place will be
18″.3 in the hypothesis of an ether; and
217.6 without it; which seems to prove the necessity of in-

troducing a correction, of which the law is not very remote from that which is here supposed.

It is only the masses of ♃ and ♀ of which the changes could have any material influence, the perturbations of the other planets being too small to require further consideration.

For Jupiter Mr. Bouvard finds, from the motions of ♄ $\frac{1}{1070 \cdot 5}$.

Mr. Gauss and Mr. Nicolai, from Pallas and Juno $\frac{1}{1053 \cdot 924}$, and this latter value is confirmed by Vesta and Ceres, of which the elements have lately been corrected.

The agreement of the observations of the comet will be still more satisfactory if we employ a mass greater than $\frac{1}{1067 \cdot 09}$, and if we substitute the mass of Nicolai we shall have

	Diff. A. R.	Decl.
1819,	+ 26″.1	+ 30″.2
	+ 5.2	+ 15.6
	− 20.4	− 1.5
1822,	+ 38.8	+ 26.4
	− 17.8	− 0.4
	+ 2.3	− 0.3
1825,	− 25.9	+ 16.1
	− 14.8	+ 8.4
	− 1.1	+ 2.2
	+ 2.6	− 5.3
	+ 18.5	− 9.2
	+ 12.9	− 13.8
1828,	− 20.0	+ 17.7
	+ 18.0	− 6.4
	+ 3.8	− 16.8
	− 1.7	− 11.0
	− 2.9	+ 3.3

of which the sum of the squares is 7775″.

For Venus, Mr. Burckhardt has found a diminution of $\frac{1}{9}$, or $\frac{\delta ♀}{♀} = -0.1125$. The observations of the comet would give us $\frac{\delta ♀}{♀} = -0.0583$: but if we employ the mass found by Burckhardt, the sum of the squares of the errors is neither increased nor diminished.

Without wishing to decide that it is necessary to employ for the comet the mass of ♃ such as Mr. Nicolai has found it, the last results have been added in order to prove that the reality of the hypothesis of an ether does not depend on the supposed value of the mass of any of the planets.

Dated, *Berlin*, 7 *Feb.* 1829.

---

iv. LUNAR OCCULTATIONS *of the Principal Fixed Stars, in the months of April, May, and June,* 1829, *calculated for the Royal Observatory at Greenwich. By* THOMAS HENDERSON, *Esq.*

Date.	Names of Stars.	Magnitude.	Immersion.		Emersion.	
			Mean Time.	Point.	Mean Time.	Point.
			H. M.	°	H. M.	°
April 10	*k* Geminorum	5.	13 11	8 L.	Under H	orizon.
May 29	ξ^1 Arietis	6.	Under H	orizon.	15 13	107 R.
June 13	μ Libræ	5.6	10 29	131 L.	11 31	133 R.

The explanations of the columns are the same as in preceding numbers.

Observers are advised to be prepared two or three minutes before the times here stated, the calculations having been performed in an approximate manner only.

---

v. *Extract of a Journal of the* TIDES, *kept at* ST. HELENA, *by Lieut.* JOHNSON, R.N. *Communicated by the Rev.* FEARON FALLOWS, M.A.

[The original Observations are deposited in the Library of the ROYAL SOCIETY.]

High and Low Water, the Rise and Fall of the Tides at St. Helena, during NOVEMBER, 1826. In James's Bay.

DATE.	Time of High Water.		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
NOVEMBER.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
1	3	25 A	1	7¼	10	0 A	1	11¼	3	6½
	4	10 P	1	4¼	10	30 P	1	11½	3	3¾
2	4	45 A	1	4½	10	35 A	1	11	3	3½
	4	45 P	1	2	10	52 P	1	11¼	3	1¼
3	4	45 A	1	1¾	11	0 A	1	11	3	0¾
	5	10 P	..	1½	10	25 P	1	8¼	2	9¾
4	4	35 A	..	10½	10	52 A	1	8	2	6½
	5	15 P	..	10¾	10	45 P	1	2½	2	1¼
5	5	0 A	..	10½	11	25 A	1	6½	2	5
	6	20 P	..	8	1	30 A	1	0¾	1	8¾
6	8	0 A	..	4½	2	20 P	1	2	1	6½
	8	35 P	..	5	3	0 A	1	2¼	1	7¼
7	9	20 A	..	11¾	3	25 P	1	1½	2	1¼
	9	35 P	..	7	3	15 A	..	9¼	1	4¼
8	9	20 A	..	3¼	3	25 P	..	9¾	1	1
	9	30 P	..	6	3	42 A	1	2	1	8
9	10	0 A	..	7½	4	20 P	1	2¼	1	9¾
	10	30 P	..	7¾	4	45 A	1	2	1	9¾
10	10	25 A	..	8	5	10 P	1	4¾	2	0¾
	11	45 P	..	9	5	56 A	1	5¾	2	2¾
11	0	15 P	..	8½	6	52 P	1	4¾	2	1¼
12	1	0 A	..	9	7	10 A	1	5¼	2	2¼
	1	25 P	..	9¼	7	40 P	1	8¼	2	5½
13	1	56 A	..	9	7	45 A	1	8¼	2	5¼
	1	20 P	..	8¼	7	32 P	1	6¾	2	3
14	1	40 A	..	10¾	8	0 A	1	8¼	2	7
	2	5 P	..	9¾	8	15 P	1	8	2	5¾
15	2	25 A	..	11¼	9	0 A	1	8¾	2	8
	3	20 P	..	9¼	9	52 P	1	9	2	6¼
16	4	0 A	..	10	9	45 A	1	8½	2	6½
	3	50 P	..	9¼	10	0 P	1	6	2	3¼
17	4	10 A	..	8¾	10	25 A	1	7¼	2	4
	4	46 P	..	8¾	10	55 P	1	4¾	2	1½
18	5	0 A	..	8½	11	5 A	1	7½	2	4
	5	20 P	..	8	11	25 P	1	5½	2	1½
19	5	35 A	..	7¾	11	45 A	1	6	2	1¾
	5	52 P	..	6¼	Midnight.		1	2¾	1	9
20	6	0 A	..	6½	0	10 P	1	2¾	1	9¼
	6	15 P	..	6½	0	25 A	1	0½	1	7
21	6	35 A	..	6½	0	52 P	1	1½	1	8
	7	10 P	..	6	1	15 A	1	2	1	8
22	7	35 A	..	6¾	1	56 P	1	2¼	1	9
	8	20 P	..	7	2	45 A	1	1¾	1	8¾
23	9	0 A	..	5¾	3	15 P	1	2	1	7¾
	9	22 P	..	7¾	3	30 A	1	0	1	7¾
24	9	45 A	..	6	4	0 P	1	2¾	1	8¾
	10	15 P	..	10½	4	35 A	1	2¼	2	0¾
25	11	15 A	..	7¼	5	25 P	1	6	2	1¼
	11	50 P	1	0	6	5 A	1	6½	2	6½
26	0	25 P	..	10¾	6	37 P	1	8½	2	7¼
27	0	52 A	..	1¾	7	0 A	1	9	2	10¾
	1	10 P	..	11¾	7	25 P	1	10	2	9¾
28	1	50 A	1	2¾	8	15 A	1	10¾	3	1½
	2	15 P	1	2	8	25 P	1	11¾	3	1¾
29	2	42 A	1	4¼	8	54 A	2	0¼	3	4½
	2	25 P	1	2½	8	51 P	2	0	3	2½
30	3	0 A	1	4	9	10 A	2	1	3	5
	3	25 P	1	3¾	9	30 P	2	0¼	3	4

High and Low Water, the Rise and Fall of the Tides, at St. Helena, during DECEMBER, 1826.

DATE.	Time of High Water.		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
DECEMBER.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
1	3	45 A	1	3½	10	0 A	2	0½	3	4
	4	10 P	1	3½	10	25 P	1	10	3	1½
2	4	20 A	1	2¾	10	35 A	1	11½	3	2¼
	4	56 P	1	3	10	0 P	1	10½	3	1½
3	5	5 A	1	2½	11	15 A	1	9¾	3	0¼
	5	25 P	1	2	11	40 P	1	11	3	1
4	6	10 A	1	1	0	30 P	1	6¼	2	7¼
	6	45 P	..	11	0	58 A	1	5	2	4
5	7	10 A	..	10¾	1	25 P	1	4½	2	3¼
	7	45 P	..	8	1	54 A	1	4¾	2	0¾
6	8	0 A	..	7¾	2	10 P	1	5	2	0¾
	8	32 P	..	6	2	45 A	..	11¼	1	5¼
7	9	0 A	..	5¼	3	12 P	1	2½	1	7¾
	10	0 P	..	4¾	4	10 A	1	0	1	4¾
8	10	30 A	..	2½	4	53 P	..	11	1	1½
	11	15 P	..	5¼	5	30 A	1	2¼	1	7½
9	11	38 A	..	6	5	57 P	1	1	1	7
10	0	0 A	..	5¼	6	10 A	1	1	1	6¼
	0	22 P	..	5	6	40 P	1	1½	1	6½
11	0	57 A	..	6½	7	20 A	1	3¾	1	10¼
	1	30 P	..	7	7	42 P	1	6	2	1
12	1	54 A	..	8	8	0 A	1	7	2	3
	2	0 P	..	7¾	8	5 P	1	8	2	3¾
13	2	12 A	..	8¾	8	30 A	1	8½	2	5¼
	2	41 P	..	8¾	8	52 P	1	8	2	4¾
14	3	0 A	..	9	9	5 A	1	8½	2	5½
	3	15 P	..	10	9	26 P	1	8¼	2	6¼
15	3	44 A	..	10½	9	53 A	1	7¾	2	6¼
	4	10 P	..	11	10	16 P	1	9	2	8
16	4	25 A	..	10½	10	43 A	1	9¼	2	7¾
	5	0 P	..	10¾	11	5 P	1	10¼	2	9
17	5	13 A	..	11	11	23 A	1	9¾	2	8¾
	5	35 P	..	8½	11	48 P	1	6½	2	3
18	7	0 A	..	8¾	1	10 P	1	8	2	4¾
	7	22 P	..	8¼	1	35 A	1	8	2	4¼
19	7	53 A	..	8	2	10 P	1	8¾	2	4¾
	8	16 P	..	8½	2	22 A	1	9	2	5½
20	8	36 A	..	8¼	2	53 P	1	8¾	2	5
	9	9 P	..	7	3	23 A	1	7¼	2	2¼
21	9	40 A	..	7¾	3	54 P	1	6½	2	2¼
	9	20 P	..	8¼	3	30 A	1	0¾	1	9
22	9	38 A	..	4	3	51 P	1	1½	1	5½
	10	5 P	..	8	4	11 A	1	2	1	10
23	10	22 A	..	7½	4	34 P	1	2¼	1	9¾
	10	48 P	..	8¾	4	58 A	1	2	1	10¾
24	11	8 A	..	4½	5	20 P	1	3	1	7½
	10	35 P	..	6½	4	45 A	1	5¼	1	11¾
25	11	0 A	..	9	5	15 P	1	9¾	2	6¾
	10	32 P	..	8½	4	50 A	1	9½	2	6
26	11	15 A	..	11	5	37 P	1	10	2	9
	10	55 P	1	0	5	8 A	1	10½	2	10½
27	11	35 A	1	1	5	58 P	1	9¼	2	10¼
28	0	10 A	1	1¾	6	25 A	1	9¼	2	11
	..	..	1	2	..	..	1	10	3	0
29	..	..	1	2½	..	..	1	10½	3	1
	..	..	1	3	..	..	1	11	3	2
30	..	..	1	4¾	..	..	1	8	3	0¾
	..	..	1	4	..	..	1	7	2	11
31	4	31 A	1	2	10	42 A	1	6	2	8
	4	51 P	1	1¾	10	10 P	1	4¾	2	6½

TIMES OF HIGH and LOW WATER, RISE and FALL of TIDES, at ST. HELENA, during JANUARY, 1827.

DATE.	Time of High Water.		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
JANUARY.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
1	4	53 A	1	1½	11	0 A	1	4	2	5½
	5	16 P	1	1	11	25 P	1	4	2	5
2	5	36 A	1	1	11	45 A	1	4	2	5
	6	0 P	1	0¾	0	10 A	1	7	2	7¾
3	6	20 A	..	11	0	36 P	1	4	2	3
	6	51 P	..	10	1	0 A	1	3½	2	1½
4	7	15 A	..	10	1	30 P	1	4	2	2
	7	42 P	..	6½	1	55 A	1	4½	1	11
5	8	10 A	..	3¼	2	20 P	1	1¾	1	5
	8	32 P	..	5	2	48 A	0	10½	1	3½
6	8	56 A	..	5¼	3	15 P	1	1	1	6¼
	9	35 P	..	3¾	4	0 A	1	1	1	4¾
7	10	25 A	..	3¼	4	38 P	1	1	1	4¼
	10	51 P	..	4¼	5	10 A	1	0¾	1	5
8	11	25 A	..	3¼	5	51 P	1	1	1	4¼
9	0	10 A	..	5¼	6	21 A	1	2	1	7¼
	0	32 P	..	5	6	43 P	1	1	1	6
10	0	54 A	..	7	7	10 A	1	5¾	2	0¾
	1	25 P	..	6¼	7	40 P	1	7	2	1¼
11	1	51 A	..	7	8	0 A	1	7½	2	2½
	2	10 P	..	10	8	21 P	1	7¾	2	5¾
12	2	32 A	..	10¾	8	45 A	1	8¼	2	7
	2	58 P	..	11½	9	5 P	1	5¾	2	5¼
13	3	15 A	1	0	9	23 A	1	8½	2	8½
	3	35 P	1	0	9	47 P	1	8	2	8
14	3	55 A	1	0½	10	5 A	1	5¾	2	6¼
	4	17 P	1	2	10	25 P	1	7	2	9
15	4	31 A	1	1¼	10	42 A	1	9½	2	10¾
	4	51 P	1	2	11	0 P	1	9½	2	11½
16	5	7 A	1	1¾	11	12 A	1	7½	2	9¼
	5	20 P	1	2½	11	31 P	1	4½	2	7
17	5	43 A	1	1¼	11	54 A	1	7	2	8¼
	6	5 P	1	2½	0	20 A	1	4½	2	7
18	6	31 A	1	1¼	0	42 P	1	5½	2	6¾
	6	55 P	1	0½	1	7 A	1	1¼	2	1¾
19	7	17 A	1	0	1	25 P	1	2¾	2	2¾
	7	33 P	..	9¼						
20	7	55 A	..	9¾	2	17 P	1	2½	2	0¼
	8	50 P	..	8¼	3	10 A	..	11¾	1	8
21	9	25 A	..	8	3	52 P	1	2	1	10
	10	8 P	..	9¼	4	25 A	1	2¼	1	11½
22	10	38 A	..	9	4	50 P	1	2½	1	11½
23	11	15 A	..	8¼	5	32 P	1	4¼	2	0½
24	12	m	..	10	6	15 P	1	6	2	4
25	1	0 P	1	2	7	21 P	1	6	2	8
26	1	36 A	1	2½	7	50 A	1	9¼	2	11¾
	2	5 P	1	4¼	8	22 P	1	7	2	11¼
27	2	40 A	1	3¾	8	52 A	1	10½	3	2¼
	3	6 P	1	6¼	9	20 P	1	11¼	3	5½
28	3	37 A	1	4½	9	54 A	1	11¼	3	3¾
	4	10 P	1	7	10	22 P	1	8½	3	3½
29	4	38 A	1	6½	10	54 A	1	9¼	3	3¾
	.	..	1	7	11	14 P	1	8¾	3	3¾
30	5	23 A	1	6	11	35 A	1	8¾	3	2¾
	5	48 P	1	5	0	2 A	1	2½	2	7½
31	6	13 A	1	4	0	30 P	1	6¾	2	10¾
	6	45 P	1	2½						

TIMES of HIGH and LOW WATER, RISE and FALL of TIDES, at ST. HELENA, during FEBRUARY, 1827.

DATE.	Time of High Water		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
FEBRUARY.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
4	..	..	..	..	1	35 A	..	10	..	..
	7	14 A	..	3¼	1	20 P	..	10	1	1¼
	7	25 P	..	3	..	..	..	9	1	0
5	8	35 A	..	4½	2	48 P	..	8¼	1	0¾
	9	0 P	..	4	3	15 A	..	7¾	..	11¾
6	9	34 A	..	5¾	3	43	..	8	1	1¾
	9	54 P	..	6½	4	0 A	..	9¼	1	3¾
7	10	10 A	..	7	4	19 P	..	9¾	1	4¾
	10	34 P	..	8¼	..	..	..	11	1	7¼
8	10	50 A	..	8¼	5	0 P	1	2	1	10¼
	11	15 P	..	9¼	..	..	1	3	2	0¼
9	..	..	..	11	7	13 P	1	2	2	1
10	..	..	1	1	8	17 A	1	5	2	6
	1	47 P	1	2½	7	53 P	1	4¼	2	6¾
11	2	1 A	1	1¾	8	15 A	1	7¾	2	9½
	2	27 P	1	5	9	33 P	1	4½	2	9½
12	3	47 A	1	5	9	58 A	1	7	3	0
	4	12 P	1	6¼	10	22 P	1	5½	2	11¾
13	4	35 A	1	6¼	..	..	1	7½	3	1¾
	..	..	1	6½	10	10 P	1	5½	3	0
14	4	5 A	1	6	10	24 A	1	8½	3	2½
	4	25 P	1	7	10	10 P	1	8½	3	3½
15	4	41 A	1	4½	10	55 A	1	8½	3	1
	5	5 P	1	5½	..	..	1	8	3	1½
16	..	..	1	5½	11	39 A	1	7	3	0½
	5	44 P	1	3	..	..	1	6½	2	9½
17	6	19 A	1	2½	0	32 P	1	5¾	2	8¼
	..	..	1	3½	1	3 A	1	4½	2	8
18	7	23 A	1	1	..	..	1	3	2	4
	7	59 P	1	0	..	..	1	3	2	3
19	8	42 A	..	10	..	..	1	2	2	0
	9	21 P	..	9	3	39 A	1	2	1	11
20	..	..	..	8	..	..	1	3½	1	11½
	10	31 P	..	7½	4	49 A	1	2	1	9½
21	..	..	..	8	..	..	1	3	1	11
	11	49 P	..	6	6	2 A	1	4	1	10
22	..	..	..	7	6	37 P	1	4½	1	11½
23	0	58 A	..	6	..	..	1	3½	1	9½
	1	37 P	..	8½	..	..	1	4	2	0½
24	..	..	..	10	..	..	1	4½	2	2½
	..	..	..	11	8	15 P	1	5	2	4
25	2	18 A	1	3¼	..	..	1	7	2	10½
	..	..	1	7½	..	..	1	7	3	2½
26	2	21 A	1	7	..	..	1	6	3	1
	2	36 P	1	7½	8	47 P	1	6	3	1½
27	3	20 A	1	5	9	35 A	1	10½	3	3½
	3	41 P	1	7½	..	..	1	5½	3	1
28	4	9 A	1	4½	10	20	1	9½	3	2
	..	..	1	7	10	47 P	1	3½	2	10½

TIMES of HIGH and LOW WATER, RISE and FALL of TIDES, at ST. HELENA, during MARCH and APRIL, 1827.

DATE.	Time of High Water.		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
MARCH.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
1	5	9 A	1	3½	11	11 A	1	6	2	9½
	5	32 P	1	4	11	46 P	1	5	2	9
2	6	0 A	1	2	0	14 P	1	2¾	2	4¾
	6	27 P	1	1	0	46 A	1	3½	2	4½
3	7	10 A	..	11	1	27 P	1	1	2	0
	7	41 P	..	10	2	0 A	..	9	1	7
4	8	17 A	..	7	2	39 P	..	11	1	6
	8	59 P	..	6½	3	17 A	..	10	1	4½
5	9	27 A	..	5½	3	42 P	..	11	1	4½
	9	58 P	..	5¾	4	16 A	..	11¼	1	5
6	10	37 A	..	5	4	32 P	..	10½	1	3½
	11	5 P	..	4½	5	19 A	..	11	1	3½
7	11	21 A	..	4½	5	31 P	..	11¼	1	3¾
	11	49 P	..	5	5	59 A	1	0	1	5
8	..	..	..	6½	..	..	1	1	1	7½
	11	27	..	8	5	36 A	1	1½	1	9½
9	..	..	..	8½	5	58 P	1	1	1	9½
10	0	9 A	..	9	..	..	1	1¾	1	10¾
	..	..	..	..	6	51 P	1	3	..	..
11	..	..	1	1½	7	27 A	1	5½	2	7
	..	..	1	1	8	0 P	1	6	2	7
12	..	..	1	4	8	19 A	1	7	3	1
	2	34 P	1	6	8	50 P	1	9	3	3
13	3	1 A	1	5¾	9	11 A	1	9	3	2¾
	3	19 P	1	6½	..	..	..	..	..	..
APRIL.										
11	3	5 P	1	6	..	..	1	8	3	2
12	..	..	1	6	..	..	1	7	3	1
	3	36 P	1	4½	10	26 P	1	5¾	2	10¼
13	..	..	1	5	10	41 A	1	7	3	0
	4	17 P	1	4½	..	..	1	8¼	3	0¾
14	..	..	1	5	..	..	1	7½	3	1
	..	..	1	3	..	..	1	6½	2	9½
15	5	19 A	1	2½	..	..	..	..	..	..
16	6	38 A	1	0	0	53 P	1	4½	2	4½
	..	..	..	11	..	..	1	3	2	2
17	..	..	..	10	..	..	1	2	2	0
	..	..	..	8	3	50 P	..	11½	..	..
18	9	31 P	..	4½	..	..	..	11	1	3½
19	..	..	..	5	4	22 P	..	10	1	3
	..	..	..	5¾	..	..	1	0	1	5¾
20	..	..	..	8	..	..	1	0	1	9
	..	..	..	8½	6	14 A	1	2	1	10½
21	0	38 P	..	8¾	..	..	1	2	1	10¾
22	..	..	1	0½	..	..	1	2½	2	3
	1	22 P	1	3	7	33 P	1	2	2	5
23	..	..	1	2½	7	55 A	1	4	2	6½

TIMES of HIGH and LOW WATER, RISE and FALL of TIDES, at ST. HELENA, during MAY, 1827.

DATE.	Time of High Water.		Rise of Tide.		Time of Low Water.		Fall of Tide.		Height of Tide.	
MAY.	H.	M.	F.	I.	H.	M.	F.	I.	F.	I.
8	..	..	..	..	7	0 P	1	8	..	..
9	1	10 A	1	3¾	7	18 A	1	7	2	10¾
	1	38 P	1	4½	..	..	1	7¼	2	11¾
10	..	..	1	5¾	..	..	1	6	2	11¾
	..	..	1	3	..	..	1	4	2	7
11	..	..	1	3½	9	10 A	1	4¾	2	8¼
	3	12 P	1	2¾	9	23 P	1	5	2	7¾
12	..	..	1	3	..	..	1	3½	2	6½
	3	52 P	1	2	10	5 P	1	2½	2	4½
13	..	..	1	1	..	..	1	3	2	4
	5	10 P	1	2	..	..	1	3	2	5
16	..	..	..	..	1	14 P	1	0¾	..	..
	7	25 P	..	11	1	38 A	1	0	1	11
17	..	..	..	9½	..	..	..	11	I	8½
	..	..	..	9	..	..	..	..	..	..
18	..	..	..	..	3	45 P	..	10½	..	..
20	11	41 A	..	9½	..	..	..	..	..	..
22	1	9 P	..	10½	7	20 P	1	1¾	2	0¼
	1	31 A	..	10¾	7	38 A	1	3	2	1¾
23	1	57 P	..	10¾	8	10 P	1	3	2	1¾
	2	21 A	..	10½	..	..	1	3	2	1½
24	..	..	..	10¾	8	30 P	1	3	2	1¾
	2	47 A	..	10½	9	4 A	1	3¼	2	1¾
25	3	14 P	..	10¾	9	19 P	1	4¾	2	3½
	3	30 A	..	10¾	9	27 A	1	4	2	2¾
26	3	26 P	..	11	..	..	1	6	2	5
	..	..	..	10¼	10	5 A	1	3½	2	1¾
27	4	8 P	..	11	..	..	1	4¾	2	3¾
	4	15 A	1	2½	10	27 A	1	2½	2	5
28	4	25 P	..	10¼	10	32 P	1	2¼	2	0½
	4	41 A	..	9½	10	51 A	1	1¾	1	11¼
29	5	0 P	..	9¾	11	11 P	1	4½	2	2¼
	5	16 A	..	8¾	11	26 A	1	1	1	9¾
30	..	..	..	9	11	49 P	1	3	2	0
	..	..	..	8¾	..	..	..	11½	1	8¼
31	6	37 P	..	8¾	..	..	..	..	..	..

(To be Continued in our next Number.)

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Ascent of Hot Air through Tubes.*—A very extensive series of experiments has been made on the velocity with which hot air ascends through tubes, the object being to ascertain the principal points which are necessary to be considered in the construction of chimneys: for this reason, the experiments were made as much as possible to resemble the state of things in actual flues, and the results are in consequence directly applicable, and of an increased value. The account of the whole is published, in a *Traité de la Chaleur*, 2 vols. 8vo., and also in the *Annales de l'Industrie*, ii. p. 110. The conclusions, for which only we have room, are as follow:—

i. The tunnels or flues oppose a resistance to the motion of hot air through them, in the direct proportion of the length of the tunnel and the square of the velocity, and inversely as the diameter. ii. The co-efficient of friction is not the same for different substances. iii. On contracting the upper orifice, the velocity of the air at the orifice increases to a certain limit, which is the velocity due to the pressure existing in the lower part of the tunnel. iv. On contracting the lower orifice, the quantity passed diminishes only in the proportion of the diameter of the orifice, and, consequently, the velocity in the orifice itself increases in the inverse ratio of its diameter.

The two last results will have numerous applications in the arts. A powerful draught is often indispensable, and is always useful in economizing the fuel. Up to this time, only two elements of draught have been considered,—the height of the chimney, and the temperature of the hot air; but the height is increased only at a serious expense, and to increase the temperature of the air requires the consumption of fuel. From the facts stated in the memoir, it appears that the diameter of the chimney is also an important element of draught; limited when the upper orifice is constant, but indefinite when that is free, and costing but little in construction. Thus, by augmenting the diameter of chimneys, and leaving the upper orifice free, or of a constant diameter, an increased draught may be obtained even when the temperature of the ascending column is diminished.

2. *On the Construction of Vitro-Crystalline Object Glasses.*—Achromatic object glasses for telescopes are constructed usually of two or more lenses, some of which are of flint glass, the others of crown glass, the achromatic effect depending upon the arrangement of two different media, possessing different dispersive powers. The scientific world is well acquainted with the endeavours which have been made, for years past, to improve the heavier of these two glasses, namely, the flint glass. In the mean time, M. Cauchoix, of

Paris, has been engaged in examining the improvement which might be effected by replacing the crown glass, and at last confidently recommends rock crystal for this purpose; by the use of which, he says, he can, with the same magnifying power, augment the light, and diminish the length of the telescope, without much increasing the price of the instrument.

By comparing the best telescopes of Dollond, made in 1758, of three and a half inches aperture and forty-two inches focal length, with what may be obtained by using rock crystal instead of crown glass, he concludes that the length may be shortened to twenty-eight inches three lines, and even to twenty-five inches, by the use of Guinand's glass, at the same time that the same magnifying power is obtained, and the clearness or quantity of light augmented. In terrestrial telescopes, a still greater reduction than this may be effected, amounting even to one half the whole length, and one eighth of the diameter.

The hardness of rock crystal, although it will make more work necessary, will, at the same time, secure the telescope from injury, as it will always replace the external glass; and so, whilst it resists itself, the wear and tear of use will protect the soft glass within. As to the difficulty of obtaining pieces of sufficient size, M. Cauchoix thinks that, when a demand is created for them, they will be found and brought into the market. He has actually constructed an instrument of thirty lines clear aperture and eighteen inches long, which has been substituted with great advantage for another of twenty lines in aperture, and equal length attached to a theodolite. Stars are seen by the former, which were invisible with the latter. He has also constructed a great number of smaller size, many of which having been tried both on land and at sea, have been found equally efficient with ordinary telescopes of double their length. —*Ann. de l'Industrie*, ii. 34.

3. *Zenneck's Pycnoscope, or Instrument to measure the Density of Solid Bodies.*—The instrument consists of a cylindrical glass vessel, surmounted by a plate and a graduated glass tube. The cylinder must be six or seven times larger in diameter than the tube, and is to be closed above by a ground flat plate of glass or metal, which may be applied to the edges of the cylinder with pomatum, so as to render the junction perfectly air and water tight. This plate has a hole in the centre to receive the tube, which is also to fit accurately. A ring or plate of lead is also to be provided, to be put upon the plate, and prevent it from rising when the apparatus is filled with water. A glass vessel or flask is to accompany this apparatus, competent to hold an equal quantity of water with the cylinder. When this instrument is to be used, the substance, whose specific gravity is to be examined, is to be put into the dry pycnoscope, its weight ascertained, and then the measure of water poured in. The height to which this rises in the graduated tube, shews the volume

of the body under examination; which, with the weight, gives the specific gravity.—*Kastner's Archives*, xiv. 81.

4. *Method of preparing Flax at Goldkronach, in Bavaria.*—Successive layers, an inch in thickness, of straw, beech ashes, and flax which has been hackled once, are put into a tub or other vessel; the different layers being separated by cloths. The whole is covered over with a double cloth; and upon that, a layer of ashes, a handbreadth in thickness. This arrangement is to be watered every twelve hours, at first with cold water, which, some time after, is to be drawn off, and again used. The waterings are to be repeated twelve times, and the temperature raised each time—the three last applications being with boiling liquor. After the twelfth time, the flax is to be left for twelve hours in the solution; after which, the vessel, and its contents, is to be taken to running water, the flax carefully washed, beaten on a flat stone with a mallet having a large surface, re-washed, pressed, and then passed through a solution of black soap. It is to be dried in a quiet, shady place; being spread out in thin layers upon canvass, and moved from time to time until dry. It is to be beaten again with the mallet, and then hackled to the degree required. Experience has proved that, by this means, the roughest and most friable flax becomes as soft as silk. The whole operation consists in a careful application of an alkaline lye, which, dissolving out the gummy and resinous matter, removes that which had rendered the fibres brittle, and makes them, in consequence, both finer and softer. —*Bull. Univ.*, D. x. 279.

5. *Alloyed Iron Plate.*—A manufacture of prepared iron has been practised, and the substance produced used, to a considerable degree, in Paris. The object has been to prepare iron in large plates, and other forms, so that it will not rust; and this has been effected by coating it with an alloy of tin and much lead, so as to form an imitation of tin plate. The trials made with this article have been favourable; it very well resists the action of certain fluids that would rapidly corrode iron alone, and can be prepared of any size, and at a low price. Its use in the manufacture of sugar pans and boilers, in the construction of roofs and gutters, is expected to be very considerable.—*Bull. d'Encouragement.*

6. *Cohesion of Iron and Steel.*—According to the experiments of M. de Mittis, a bar of good Styrian iron, one inch square, was broken by a weight of 400 quintals; a similar bar of Styrian steel, not hardened, was broken by a weight of 749.53 quintals; a third and similar bar of the meteoric steel, from the manufactory of M. Fischer, at Hainfeld, in Austria, was broken by a weight of 1130 quintals.—*Kastner's Archives*, xiv. 175.

7. *Fire Arms.*—It is proposed by M. Burel to fix a small mirror, 0.47 of an inch, in the side, near the mouth of the piece, so that the person using it shall see the reflection of his own eye. In this way it is supposed that very exact aim may be taken; and the experiments made by various officers and sportsmen seem to encourage the idea that this application may be useful. It is considered as most likely to prove serviceable when applied to war pistols.—*Bull. d'Encouragement.*

8. *Preparation of Hydraulic Lime.*—It was observed by M. Lafaye, and confirmed by M. Vicat, that lime which had been slaked by immersion, may be preserved for a long time without alteration.

MM. Ollivier, assisted by M. Vicat, have succeeded in preparing a lime which they can preserve for a long time in sacks, without losing any of its properties, and at the same price with unslaked lime. The use of this lime is said to be very advantageous; it avoids any difficulty in the measurement, and is more easy of transference. Its use in preparing walls for fresco painting, is superior to all other lime.

It is sold in very fine powder; and, having been sifted, is free from vitreous particles. These operations, as well as the moving it into the sacks, is not at all inconvenient to the workmen, who continue for twelve hours per day thus occupied, without injury. From one to one and a half millions of sacks of this lime are sold annually, being principally employed in the construction of canals in Bretagne.—*Bull. d'Encouragement.*

9. *On Hydraulic Cements.*—A paper is contained in the Jern-Kontorets Annaler of Sweden, for 1824, on hydraulic cements, by M. Pasch, who gives in it an account of the experiments that had been made at the Canal de Goetha. After many experiments upon the substances which most importantly improved lime by mixture with it, he concluded that the most powerful was an aluminous schist. It would be difficult to find a lime, he says, which would not be improved by mixture with this schist. For application it is to be burnt and then reduced to powder: although the carriage to a distance makes this preparation expensive, yet its good qualities are supposed fully to compensate.

No good effect was found to be produced by the use of manganese in any way; nor by the use of trap rock, green-stone, calcined granite powder, or ochre.

Relative to lime-stones those are not the best which are purest, but the reverse. A considerable quantity of argillaceous earth in limestone makes the cement formed with it set well in water; on the contrary, siliceous earth gives greater hardness to the mortar, but it does not then so well resist water. Bituminous limestones were found to be the best, perhaps, because those used were mingled with aluminous schist. The cement made with this stone set in a few minutes, and soon acquired the hardness of stone, to such a degree, that the substance might be used in place of Parker's cement.—*Bull. Univ.* E. x. 343.

10. *Altitude of certain Buildings*:—

	meters.
The highest Pyramid in Egypt	146
The Cathedral at Anvers	144
The Cathedral at Strasburg	142
The Steeple of St Stephen, at Vienne, (Austria)	138
The Steeple of St. Martin, at Landshut	137
St. Peter's, at Rome	132
The Steeple of St. Michael, at Hamboro'	130.5
The Steeple of St. Peter, at Hamboro'	119
St. Paul's Cathedral, at London	109.7
The Cathedral of Ulm	109.4
The Cathedral of Milan	109
The Tower of the Asinelli, at Bologna	107
The Dome of the Invalids, at Paris	105
The Cathedral of Magdebourg	101.6
The Cupola of the Pantheon, at Paris	79
The Balustrade of Notre Dame, at Paris	66

The metre is 39.37 inches.—*Bull. Univ.* F. xv. 257.

11. *Prevention of Fires in Theatres.*—In consequence of the frequent occurrence of fires in theatres, particular precautions have been taken with the theatre of the Port St. Martin, at Paris. A thick wall of hewn stone separates the audience part from the scenic part of the house; all the doors in it are of iron, and may be shut instantly, in case of fire; finally, the insulation of the spectators from the stage is made perfect by means of a screen of plates of iron, which falls down before the stage. This screen, which weighs between 1,200 and 1,300 pounds, is easily worked by two men, and slides up and down upon guides, so as readily to take its place. Besides these precautions, reservoirs of water are established in the roof, which may be connected, when necessary, with vessels of compressed air, and made to throw a powerful jet over a very large part of the building.—*Moniteur. Bull. Univ.* E. x. 333.

12. *Influence of Metals on the Motion of magnetised Bodies, &c.*—Whilst experimenting upon the extraordinary effect of metals upon magnets, discovered by M. Arago, M. Seebeck observed, amongst a number of other effects, now well known to the public, that if a magnetic needle be suspended horizontally above a metallic plate, and made to oscillate, not in the ordinary way, but as a pendulum, still the same influence of the plate beneath will be exerted upon it as in the usual experiment, and the number of oscillations will be

fewer than if no metal plate had been there. He also observed, that, if a copper pendulum be made to vibrate above a magnet, or between its poles, the vibrations are less in number and extent than if no magnetic influence had existed in the immediate neighbourhood of the pendulum. Amongst metals mercury is that which, as a pendulum, is least acted upon by the magnetic influence. A pendulum of wood, with a ball of marble or glass, is not at all acted upon or influenced by the magnetism either of magnets or of the earth.—*Annalen der Physik*, 1828, p. 352.

Query.—What will be effect of the earth's magnetism upon pendulums vibrating in different directions relative to the magnetic meridian, and in different latitudes upon the earth's surface?—*Ed.*

13. *Diurnal Variations of the Barometer.*—M. Bouvard has lately read a paper to the Academy of Sciences, of Paris, on the diurnal variations of the barometer. The following are the results of his observations. i. Towards the equinoxes the barometer attains its maximum at 50 minutes past 8 o'clock A.M., and at about 11 o'clock P.M. The minimum at the same period of the year is at 4 o'clock A.M. and at 4 o'clock P.M. ii. In summer the maximum is at 10 minutes past 8 o'clock A.M. and in winter at half-past 9 o'clock A.M. The other particular periods of the barometer vary equally with the seasons. iii. The day periods are longer than those of the night, and their variations are nearly like the corresponding periods. iv. Under the equator, and at the level of the sea, these periods are at their maximum. v. Under the equator, at very elevated situations, these periods diminish like the corresponding temperatures, and nearly as the square of the cosine of the latitude. According to M. Bouvard, these results will require conformation by new observations, made with the best instruments in high latitudes, and in very elevated situations under the equator.

14. *Theory of Light.*—If by means of a lens, a concave mirror, or a cylindrical glass, rays emanating from a luminous point or line are made to converge; and if two parallel surfaces, from which the polish has been slightly removed, are introduced into the converging pencil, coloured rings or bands are produced in the focus, which arise from the interference of the rays transmitted by the first surface, and dispersed by the second, with the rays dispersed at the first passage, and afterwards transmitted regularly. M. Babinet has communicated to the French Academy of Sciences some formulæ for every case which experiment affords; and in the second part of his memoir on the subject, has suggested considerations which lead to the formulæ of coloured rings of the thick plates of Newton, the theory of which had been already pointed out by Dr. Young. Several very extended notes also relate to, 1. The author's method of calculating the interferences by the versed sines of very small angles, which give immediately the differences of the paths described by the rays. 2. A simple demonstration of the

principle of Fresnel on the composition of two systems of undulations of the same length, which differ in position by any quantity. 3. On the direction of all the rays in the focus of mirrors, and simple or achromatic lenses. 4. The calculation of the fringes produced in the focus of a cylindric glass by the image of a luminous line. The integral which gives here the intensity of the light in each point of the focus is constructed geometrically, and verified by experiment.

15. *Theory of Equations.*—M. Abel, one of the most promising mathematicians of the rising generation, having investigated, has found the conditions necessary for an equation to admit of an algebraic solution; from which it results, that every equation beyond the fourth degree cannot be solved algebraically. Analysts have suspected this truth, but were not agreed respecting it. In the opinion of M. Legendre the question is now set at rest definitively.

16. A tanner, named Rapedius, of Bern Castel, on the Moselle, has discovered a new species of tan proper for dressing leather. It is the plant known by the name of Bilberry or Whortleberry (Vaccinium Myrtilus or Myrtillis), which should be gathered in spring, because at this season it dries more readily, and is more easily ground. Three pounds and a half of this tan suffice for dressing a pound of leather, while six pounds are required from the oak to produce the same effect. By this new process, tanners can gain four months out of the time required for preparing strong leather. A commission having been appointed at Treves to examine the leather so prepared, reported, that they had never seen any as good, and that every pair of shoes made therefrom lasts two months more than what are manufactured from common leather; that the skin of the neck, which it is difficult to work, becomes strong and elastic like that of the other parts. The shrub should not be pulled up, but cut with a bill, to obtain the reproduction of the plant the following year. When cut, damp does not deteriorate it, which is not the case with oak-bark, which loses ten per cent. of its value by being wetted.—*Bulletin des Sci. Tech. Jan.* 1829.

17. M. J. C. Leuchs has found in animal charcoal a method of preserving the must of grapes. With one pint of the juice 100 grains of animal charcoal, and more if the former contain much fermentable matter, are to be mixed. When it has lost its colour, and is clear, the charcoal is separated from it, and it is preserved in bottles and casks closely stopped. It does not ferment even in open vessels, because the charcoal has absorbed the principle of fermentation; this, however, has not become inactive from its combination with the first; for if the charcoal be left in the must, this

begins to ferment, but not throughout, like ordinary must in which the principle of fermentation is diffused, but only at the bottom of the vessel where the animal charcoal containing it is precipitated. —*Bull. des Sciences Agricoles, Jan.* 1829.

18. *Græco-Egyptian Papyri.*—Signor Giovanni Pelvettini, of Corfu, having translated a Greek papyrus brought from Egypt, though from what precise part is unknown, found it to be a supplication addressed to Serapis and the gods who sit with him, by Artemisia, daughter of Damasis, who intreats them to render efficacious the imprecations she utters against her father.

19. *Roman Antiquities*—At Sainte-Colombes-Vienne, department of the Rhone, some very perfect remains of Roman baths have been discovered, together with the mutilated remains of what must have been fine marble statues.

20. *Archæology.*—The Imperial Academy of Sciences at Petersburg has resolved to have undertaken at its expense a journey for the purpose of antiquarian researches throughout Russia. This interesting enterprise is intrusted to the honorary counsellor of state, M. Stroef.

21. A fine marble monument, consisting of a rectangular pedestal in grey marble, with a Latin inscription, has been discovered at Bordeaux. It belonged to a votive monument erected to *Tutela*, a mysterious goddess of the ancient city. The inscription, which is well preserved, is useful to prove the second consulship of Julianus in 224, which, although mentioned in the Capitoline marbles, has been doubted by some antiquarians. The face opposite to the inscription is decorated with a crown of oak.

II. Chemical Science.

1. *Crystallization of Ice.*—During the winter of 1821-2, Dr. Hessell observed at Lahn, near Marbourg, perfectly-formed crystals of ice, which were regular six-sided prisms, short, but with well-formed planes. This occurrence confirmed his opinion, that the crystals of ice belonged to the hexagonal or rhomboidal system; but as they did not present any planes on the borders or angles of the base, the dimensions of the prism still remain undetermined. Several of these hexagonal tables, placed one upon another, presented a grouping analogous to that assumed by the tabular calcareous spar from the Hartz.

M. Hessel also had the opportunity of remarking the crystallization of ice on his window, in 1826. The layer of ice was a quarter of an inch thick: it covered the lead as well as the glass, and instead of presenting the usual vegetative appearance, it was composed of a multitude of crystals, more or less perfect; many thousands of which exhibited the form of a hexaedral prism without any modifying faces. The axis of these crystals were perpendicular to the glass on which they were fixed. In some parts they were so grouped as to assume the hopper-like form so frequently taken by common salt.—*Kastner's Archives, Bull. Univ.* B. xv. 76.

2. *On the Evaporation of Ice, by* M. Schuebler.—The results obtained in numerous experiments being tabulated, gave the following general conclusions. The first column contains the periods during which the evaporation was observed; the second, the evaporation in twenty-four hours, estimated by the height in lines from a square foot of surface, either of ice or water; the third, the mean temperature for the period during which each experiment continued; the fourth, the mean of the hygrometer (the kind of instrument not mentioned); the fifth, the mean height of the barometer at 10° of Reaumur, or 55° F.

				Inches.	Lines.
From 1 to 10 January	0.062	17.7	64.6	26	11.59
„ 11 „ 20	0.038	17.2	62.8	27	2.37
„ 20 „ 31	0.043	18.55	62.8	27	3.62
„ 1 „ 10 February	0.100	33.5	65.0	27	3.35
„ 11 „ 20	0,122	35.92	64.9	27	2.92
„ 21 „ 28	0.343	41.2	59.3	27	4.28
Greatest evaporation in Jan. (9th)	0.233	11.8	60.0	26	10.57
Least evaporation in Jan. (6th)	0.001	27.1	67.0	26	10.26
Greatest evaporation in Feb. (26th)	0.491	30.43	57.5	27	0.91
Least evaporation in Feb. (4th)	0.016	27.5	74.0	27	2.40
Whole evaporation in January	1.480	18.0	63.4	27	1.71
Whole evaporation in February	4.97	36.7	63.3	27	3.46
The greatest evaporation in the summer of 1815, for 24 hours	76.9	76.9	30.5	27	2.35

From these observations it appears that the evaporation of ice is far more considerable than has been supposed; and that in certain natural circumstances, it may surpass even that of water. During the dry cold weather of January 9th, the evaporation from ice in 24 hours was twice as great as from an equal surface of water in the middle of February, during mild cloudy weather. The gradual disappearance of snow and ice is well known; and evidently, from the above experiment, may become very rapid in very dry weather.—*Naturwessenchaftl. Bull Univ.* A. x. 301.

3. *Conduction of Heat by Wood.*—In some experiments on this subject made by MM. Delarive and Candolle, bars of wood were used, each 13 centimeters* long, 4 wide, and 2.7 thick; 5 holes were made in each bar, two centimeters apart, and the first, 3 centimeters from the end. Each hole proceeded half way through the thickness of the wood, was 0.7 of a centimeter in diameter, and

---

* A centimeter is 0.3937 of an inch.

being filled with mercury, had the bulb of a small thermometer placed in it. One extremity of each bar was fixed in a socket of tinned iron 2.5 centimeters long, beneath which a spirit lamp with a chimney was placed, the flame being prevented from coming into contact with the wood by screens of glass. The following table shews the temperature by the centigrade scale in each hole, when the degrees of heat had become stationary; and indicate, therefore, the conducting power.

Walnut, longitudinal	80.13	43.0	19.63	9.19	5.13
Oak, ditto	81.7	41.2	17.5	7.2	3.7
Fir, ditto	84.0	39.25	20.6	8.5	3.7
Poplar, ditto	79.8	34,2	14.2	6.2	2.8
Walnut, transversal	99.5	37.43	13.9	6.0	3.25
Oak, ditto	79.3	22.75	7.5	3.6	2.4
Fir, ditto	70.9	13.8	4.5	2.5	1.9
Cork	78.5	13.75	3.44	1.56	1.0

From these results it appears, that the conductibility of wood is less transversely across the fibres than in their direction; an effect which will assist in explaining how it is that trees retain a temperature approaching to that of the soil. But at the same time it should be remarked, that surfaces equal in radiating power were not given to the specimens experimented with.—*Mem. de Geneve*, iv. 70.

4. *Chemical Action of Light.*—The following are some effects quoted by M. Fischer, as proper to be added to those which demonstrate the chemical action of light upon inorganic matter. If a solution of ferro-prussiate of potash be precipitated by alcohol, the precipitate collected and rapidly dissolved in water, and the solution exposed to light, the yellow colour soon passes to green, and ultimately Prussian blue is deposited. The solution at the same time becomes alkaline; and if the experiment be made in a close vessel, the odour of hydrocyanic acid may also be observed when the liquid is examined. The salt, in fact, is in part decomposed by the action. Prussian blue, sometimes with excess of oxide of iron, is formed and deposited, and a hydrocyanate of potash left in solution. The same changes take place with the ordinary solution of the ferro-prussiate, but more slowly. They do not take place without light.

The same salt crystallizes in large quadrangular plates in an obscure situation; but in bright light it gradually loses the power of yielding this form, and becomes dendritical and pulverulent.

The action of light upon solutions of gold and silver is very much increased by the presence of organic matters, as gum, starch, sugar, alcohol, &c. This effect, however, has been long known, and sometimes rendered available in experiments made upon the solar rays.—*Kastner's Archives*, ix. 345.

5. *Ignition of Wire by Voltaic Electricity.*—When the poles of a voltaic battery are connected by a short fine platina wire, the middle of the wire becomes red hot. This effect has generally been explained by supposing that the temperature of the wire would have been the same throughout, but for the cooling effect produced at the extremities by the contact of the masses of metal constituting the poles, in consequence of which the middle part acquired the highest temperature. M. Becquerel endeavoured to ascertain whether this was the true explanation, and therefore used a wire of such length, and placed in such circumstances, that the effect of cooling at the extremities should not interfere. It is said, that the result of this experiment shewed that the temperature continues to increase from each end to the middle of the wire; and that consequently the cause which creates an electric current, of which the intensity is constant in each point of the wire, acts as an accelerating force in the developement of heat.—*M. Mag.* vii. 94.

6. *Electricity of Woollen Cloth.*—M. Muret de Bore, a manufacturer of woollen cloth at Chateauroux, has observed that, when the pieces are dried out in the air, they frequently become powerfully electric, especially when exposed to sun-light. That which is most electric is the black cloth: slight friction upon it is sufficient to produce long electric sparks: white and sky-blue cloth do not become at all electrical; deep blue and deep green weakly so; the reds produced by cochineal and madder more so.—*Annales de l'Industrie,* ii. 319.

7. *Price of Bromine.*—Bromine has now become so abundant, that M. Quesneville, of Paris, is able to sell it for M. Balard, the discoverer, at the price of fifteen francs per ounce.

8. *Peculiar production of Ammonia.*—M. Hollunder mixed liver of sulphur and pure iron filings together, put them into a covered crucible, and exposed them to a high temperature. When the double sulphuret thus obtained was moistened with a little water, it disengaged abundant vapours of ammonia, and continued to do so, as long as it continued hot.—*Kastner's Archives, Bull. Univ.* A. x. 312.

9. *Cyanide of Sulphur.*—This compound has been formed by M. Lassaigne, by putting pulverised cyanide of mercury into a small glass globe, adding half its weight of bi-chloride of sulphur, closing the vessel, and leaving it for twelve or fifteen days in common day-light. Small crystals, amounting in weight to about one twenty-fourth part of the mixture, sublimed to the top of the globe, which, being collected, mixed with a little carbonate of lime, and re-sublimed, were pure cyanide of sulphur. This substance crystallizes in fine rhomboidal plates, resembling those of chlorate of

potash. It has a very pungent odour, acts powerfully upon the tongue, and is very volatile. By exposure to light, it becomes yellow; its aqueous solution reddens litmus, but its alcoholic solution does not, until the paper has been exposed afterwards to a moist atmosphere. Alkalies combine with it, neutralizing its striking properties. The moistened crystals put into the voltaic circuit between silver poles, are decomposed. Sulphur passes to the *positive* pole, and an odour of almonds is perceived at the *negative* pole. Potassium decomposes it, producing cyanide and sulphuret of potassium.

The substance has several of the properties of an acid. As before said, it combines with, and neutralizes alkalies and its solution, or the solution of its salts produces the same red colour with persalts of iron, as Porret's ferrochyazic acid. In certain states, indeed, it almost appears to be identical with Porret's acid, but in other points it differs; as, for instance, in its solid state, and in the action of oxide of silver upon it, which causes decomposition, abstracting the sulphur.

To ascertain the ratio of the cyanogen and sulphur in this compound, a certain weight was combined with potash, and the compound produced being mixed with six times its weight of nitre, was calcined in a silver crucible. The mass dissolved in water was neutralized by nitric acid, and the sulphuric acid formed precipitated by muriate of baryta. 100 parts of the substance gave, in one experiment, 180 of sulphate of baryta, in another 176. Calculating from this the quantity of sulphur, and, from the deficiency, the quantity of cyanogen, the compound would appear to consist of 75.8 cyanogen and 24.2 sulphur; and, therefore, the substance probably contains, according to M. Lassaigne's numbers, 4 proportionals of cyanogen, and 1 of sulphur, or 2 proportions of cyanogen 52, and 1 of sulphur 16.—*Ann. de Chimie*, xxxix. p. 197.

10. *Supposed Discovery of a method of making Diamonds.*—Much expectation has been excited by the announcement of a method of making diamonds discovered in France, and actually communicated to the Academy of Sciences, at Paris, by M. Gannal, on the 3d of November last; but, as yet, the expectation has not been satisfied, and no philosopher, who can be considered as competent to judge of the process and the products, has as yet sanctioned the announcement by the authority of his opinion. The point to be gained is the crystallization of carbon, and this, it is said, is done by the simplest possible case of chemical affinity. The well-known liquid compound of sulphur and carbon is put into a vessel, covered with a layer of water, and then a stick of phosphorus introduced; the phosphorus dissolves in the sulphuret of carbon, soon takes the sulphur to form a sulphuret of phosphorus, and the carbon is set free as a crystalline diamond powder.

It is distinctly said that crystals are obtained, white or colourless, very brilliant, hard, extremely refractive, and admitted by workers to be real diamonds.

Since M. Gannal's announcements, other diamond makers have come forward. On the 10th of November, M. Arago communicated a note to the Academy of Sciences, from M. Cagniard de la Tour, in which the latter said that he also had succeeded in crystallizing carbon and obtaining diamonds by methods different from those of M. Gannal, and that a sealed packet, left with the Secretary in 1824, contained an account of his first processes. From M. Cagniard de la Tour's character as a philosopher, we must feel satisfied he would not say so much without some good foundation; and on this ground, therefore, we have hopes that, upon inquiry, the crystallization of carbon may prove to have been effected by art.

M. Arago also announced that he knew *another person* who had arrived at similar results. As to M. Gannal's experiments, M. Gay Lussac said that, eight years ago, M. Gannal spoke to him about his attempts.

Academy of Sciences of Paris, Nov. 24th.—M. Thenard gave an account of the experiments made by himself, MM. Dumas and Cagniard de la Tour, to verify the trials, by which the latter thought he had obtained the power of crystallizing carbon, and forming diamond. An accurate analysis of these crystals, which had no colour, proved, however, that they were only silicates, and not artificial diamond.—*Ann. de Chimie,* xxxix. 327.

11. *Manufacture of Flint Glass.*—The Annales de l'Industrie Française announce the discovery of a regular process for manufacturing flint glass for optical purposes, and states that the success is certain. The process has been the result of the patient and expensive investigations made by MM. Thibeaudeau and Bontemps. The specimens of glass, both flint and crown, have been laid before the Academy of Sciences, but, as yet, no report upon their quality has been given, nor any account of the process itself published, so that opinions must remain a little longer in suspense.—Vol. ii. p. 251.

12. *Action between Metallic Chlorides and Olefiant Gas.*—M. Wöhler has remarked that, when olefiant gas is sent into liquid chloride of antimony, a very large quantity is absorbed; heat is evolved, and the fluid becomes brown. On cooling, crystals of chloride of antimony are deposited, but there is now little power of producing fumes in the air, and a strong odour of chloric ether is perceived. When distilled, a liquid product is obtained, which gradually separates into two layers, the lower being pure chloric ether, and the upper a solution of chloride of antimony in the same fluid.

The red and fluid chloride of chrome acts in a similar manner. Chloride of copper heated in olefiant gas is decomposed, and charcoal is deposited, whilst a portion of sub-chloride is formed.—*Annalen der Physik, Bull. Univ.* A. x. 258.

13. *On the Presence of Iron in Tin.*—From M. Fischer's experiments, it would appear that even the very best tin contains small quantities of iron occasionally, which, entering into the compounds afterwards formed by the metal, cannot be easily separated. The best method to decide upon its presence, is to decompose a salt formed from the tin by ammonia, and separate the protoxide which is thrown down. This precipitate is then to be digested in cold muriatic acid; nearly all the oxide dissolves; but if the portion which last remains be removed, it will be found to contain but little oxide of tin, mixed with a large proportion of oxide of iron. This portion, acted upon by warm and strong muriatic acid, will dissolve; and then the iron may be recognized in the usual manner.—*Kastner, Bull. Univ.*, A. x. 313.

14. *Supposed Presence of Mercury in Sea-water.*—Dr. Torrey, incited by the notices that at various times have appeared in journals, of the presence of mercury in sea-water, made an experiment, in which a small plate of polished gold, of known purity, was attached to the bottom of a vessel, in a situation where any great friction from the motion of the vessel was avoided. The ship sailed from New York to Liverpool, and returned to New York, when the plate of gold being examined, was found to exhibit no appearance of the presence of mercury, and to retain its original brightness.—*Silliman's Journal*, xv. 357.

Supposing that there is any other reason to suspect the presence of mercury in sea-water, we hardly think the above experiment a conclusive one. A soluble compound of mercury in sea-water would have no tendency to be decomposed by gold thus situated.—*Ed.*

15. *Method of detecting Arsenic in Sulphur.*—MM. Geiger and Reimann say, that the presence of arsenic in sulphur, to the amount only of 0.000061, may be discovered in the following manner:—A certain quantity of precipitated sulphur, flowers of sulphur, or ordinary sulphur finely pulverised, is to be digested with ammonia for a considerable time, then filtered, and afterwards the clear liquid acted upon by excess of muriatic acid. If a yellow precipitate occurs, it is an indication of the presence of arsenic; if not, the liquid is to be evaporated until only a few drops remain. A little ammonia is then to be added; afterwards, a small quantity of muriatic acid; and, finally, a little solution of sulphuretted hydrogen. If there be the smallest quantity of arsenic, it will be rendered evident by a yellow precipitate.—*Bull. Univ.*, A. x. 266.

16. *Spontaneous Inflammation of Arsenical Cobalt.*—In conse-

quence of a fire which occurred in a drug warehouse, an examination was made into the circumstances, and it was found that it arose from moist powdered arsenical cobalt, closely packed together. When this substance is pulverised, as the powder is poisonous, it is usually moistened, to prevent it from flying about. This circumstance, with the presence of the pyrites, and the closely-packed state of the whole, had produced spontaneous inflammation, the cause of which being now discovered, all druggists are warned not to keep quantities of such cobalt together in a moist state.—*Recueil Industriel*, viii. 168.

17. *Sulphate of Potash and Chrome.*—The best method of preparing this salt is to add one part of concentrated sulphuric acid to three parts of a saturated solution of chromate of potash. The precipitate at first formed will be re-dissolved, and then two parts of alcohol are to be added. The mixture soon acquires a green colour, and crystals often form immediately; if not, sufficient water may be added to dissolve all the salt separated, and the solution is to be evaporated spontaneously. To purify the crystals, they should be washed in small quantities of water, until the latter is of a pure violet colour, without any green appearance. 100 parts of this salt contain 32 sulphuric acid, 8.4 of potash, 15.6 protoxide of chrome, and 44.08 of water.

The crystals are octoedra. Solution and evaporation, at common temperatures, do not change them; but by heat, the solutions are decomposed, become green, and leave a fine green mass, or else form crystals of sulphate of potash, whilst sulphate of chrome remains in solution. A heat, from 122° to 167° F., is sufficient to effect this change perfectly, even in dilute solutions.—*Bull. Univ.*, A. x. 316.

18. *Preparation of Spongy Platina for effecting Ignition.*—M. Pleischel recommends that a piece of paper be imbibed three times in succession with a solution of muriate of platina, and then burnt. The residue is the platina, he says, in its best state. We have always found that, when prepared by heating a little pure ammonio-muriate of platina upon platina-foil in a spirit-lamp, at a temperature as low as possible, so that it be sufficient to dissipate every thing volatile, that then the platina would inflame a mixture of oxygen and hydrogen, at the lowest possible temperature.—*Ed.*

19. *Muriates of Ammonia and Platina.*—M. Fischer says, there are three of these triple salts:—that which is well known, or the neutral ammonio-muriate; another, which is white, containing excess of ammonia; and a third, which is yellow, containing excess of oxide of platina.

The white ammonio-muriate is obtained by dissolving the ordinary triple salt in ammonia, and applying slight heat. A clear, faintly yellow liquid is obtained. Alkaline carbonates, sulphates, oxalates, or phosphates, cause a white precipitate, which is the salt in question. It is obtained also by the addition of concentrated sulphuric, phosphoric, nitric, or muriatic acid. Alcohol and protosulphate of iron also produce it; but then the precipitate has to be cleared from oxide of iron by a little muriatic acid.

The yellow salt is to be prepared in a similar way. The sulphate and phosphate of soda are the best precipitants, but some hours are required for the action. A quantity of the salt to be prepared always remains in solution. Alcohol and the phosphate of soda are the best agents in this respect for the white salts; the sulphate of iron, used as mentioned, the best for preparing the yellow salt.

The following are some of the distinguishing characteristics of these three salts:—the ordinary salt crystallizes in octoedra; is soluble in 150 parts of water at common temperatures, and in 80 of boiling water; is perfectly soluble in ammonia, giving a permanent solution without colour; is insoluble in muriatic acid at common temperatures, and but little soluble by heat; is slightly soluble in dilute sulphuric acid: its solution is not affected by ferro-prussiate of soda: when heated, it does not fuse; and it evolves muriate of ammonia and chlorine.

The white salt forms an adhesive or filamentous powder; is scarcely soluble even in boiling water; is insoluble in boiling ammonia; dissolves by heat in muriatic acid, and, upon cooling, is deposited, changed into the yellow salt; is insoluble in dilute sulphuric acid; its ammoniacal solution is precipitated pale brown by ferro-prussiate of soda. When heated, it fuses, and then evolves muriate of ammonia and free ammonia.

The yellow salt forms an adhesive or filamentous powder; is less soluble than the ordinary salt at common temperatures, but very soluble at high temperatures, forming a pale turbid solution: it dissolves in ammonia by ebullition, and is converted into the white salt; it dissolves in muriatic acid without change; it dissolves in dilute sulphuric acid; its aqueous solution is precipitated yellow or brown by ferro-prussiate of soda. When heated, it first fuses, and then is decomposed, evolving muriate of ammonia, but neither free chlorine nor ammonia.—*Kastner's Archives*, xiv. 145.

20. *On Glucium and Yttrium, by* F. Wöhler.—Both of the earths glucina and yttria have been decomposed by M. Wöhler, by a process the same as that which he applied successfully to alumina[*]. The metallic bases are as little oxidable as aluminum, and have many analogies with it.

Glucinum.—The glucina was prepared from beryl, and being mixed with charcoal, and heated in a tube, chlorine, was passed over it[†], and a chloride formed; this was obtained both in white acicular crystals and in a fused mass, and was very deliquescent. This chloride was placed in layers in a platinum crucible with intervening layers of flattened potassium, the cover fixed on with a wire, and the crucible heated in a spirit-lamp. At the moment of reduction the crucible became white hot; when cold, it was opened and turned upside down into a large glass containing water, which dissolved out chlorides of potassium and glucinum, and left the metal glucinum as a greyish black powder, which was then separated on a filter, washed and dried.

---

[*] *Quarterly Journal of Science*, N. S. vol. iv. p. 209; vol. ii. p. 474.

[†] *Ibid.* vol. iv. p. 211.

In this state glucinum has the appearance of a finely-divided metal, and, when burnished, acquires a dull metallic lustre. It presents no appearance of fusion. It is not oxidized either in air or water at temperatures below 212° Fah. Ignited on platina foil it burns brightly into glucina. In oxygen gas the combustion is very vivid, but still no signs of fusion. It dissolves when heated in the sulphuric, nitric, or muriatic acid, or in solution of potash. It burns when heated in chlorine, bromine, or iodine vapour, forming volatile chloride, bromide, or iodide. It easily combines with sulphur, forming a sulphuret, a seleniuret, a phosphuret; and an arseniuret may also be formed in the same manner.

Yttrium.—The yttria was obtained from gadolinite, and converted into a chloride by charcoal, chlorine, and heat, as before. The chloride is volatile, and resembles the similar compound of glucinum. It was found exceedingly difficult to obtain the yttria pure; and, whilst forming the chloride, generally a little sulphur and potassium appeared in one or other part of the operation.

The chloride was decomposed by potassium in a crucible, as before, with vivid incandescence; and dissolving the result in water, the yttrium was obtained in small scales, having a perfect metallic lustre and grey black colour. It is easily distinguished from glucinum and aluminum by its metallic and crystalline appearance. When burnished, it acquires a perfect lustre, but its colour and metallic appearance do not equal those of aluminum—one is to the other, perhaps, as iron to tin. Aluminum appears to be a ductile metal; yttrium, on the contrary, brittle. Yttrium does not become oxidized by air or water at ordinary temperatures. Heated to redness in the air it burns into yttria: in oxygen it burns vividly, and then gives traces of fusion. It dissolves in sulphuric acid and in solution of potassa. It burns in the vapour of sulphur, producing a sulphuret. It forms also a seleniuret and a phosphuret.

From these and former experiments it results, that the bases of alumina, glucina, and yttria, are metals which, at ordinary temperatures, do not act upon air or water, but decompose water when acids or alkalies are present, and burn vividly in oxygen, chlorine, bromine, iodine, sulphur, selenium, and phosphorus.—*Ann. de Chimie*, xxxix. 77.

21. *Sulphuret of Silica.*—M. Buchner mixed equal parts of silica and sulphur by trituration; the mixture was put into a small glass

retort, and exposed to a moderate heat for several hours. The product was a porous grey mass, from which caustic potash separated only a minute portion of sulphur, and left a few grains of quartz. The alkaline solution deposited a fine black powder, which is the compound in question.—*Phil. Mag.*, N. S. ii. 234.

22. *Observations relative to Alumina, by* M. Hollunder.—The solution of alumine in nitric acid is readily decomposed by the influence of the atmosphere, even at ordinary temperatures, although elevation of temperature increases the effect; it is most rapid when free acid is present. The flocculent substance precipitated is supposed to be aluminum, oxidized to a higher degree than the alumina obtained by ordinary process; for at the same time the nitric acid undergoes decomposition, and the oxide obtained is much more insoluble than ordinary alumina. Muriatic, nitric, and sulphuric acids, and also the alkalies, only dissolve it partially, and that with difficulty, a great excess of the solvent being required.

It is supposed that a similar per-oxygenation of alumina is occasioned by other oxidizing means, such as a red heat, or the action of nitre at a high temperature, &c.—*Kastner's Archives. Bull. Univ.* A. x. 313.

23. *Manufacture of Alum.*—It has been remarked by M. D'Arcet, in regard to this manufacture, that heat has great influence over the form of the resulting salt. When cubical alum is dissolved, and its solution heated to temperatures above 110° Fah., a precipitate of sub-sulphate of alumina is produced, and the solution will now yield only octoedral crystals. Hence may be deduced the means of producing either octoedral or cubical alum at pleasure. Roman alum is crystallized in cubes, which is a consequence of the construction of the furnaces at La Tolfa. These are so constructed that they do not allow the solution to attain a higher temperature than 104° Fah.—*Industriel*, 1828.

24. *On a Test for various Organic Principles in Microscopic Chemistry, by* M. Raspail.—In a letter read to the Academy of Sciences, last October, M. Raspail describes certain effects produced by the action of sulphuric acid on vegetable matter, which he had observed by the microscope, and which he considers as enabling him to ascertain, in many cases, the chemical nature of the smallest quantity of organic matter which can thus be made visible. On placing an ovarium of *Avena sativa* in sulphuric acid, the different parts gave way very differently, some immediately dissolving without producing colour, others producing a fine purple colour, and others again not being altered in colour or acted upon in two days. Being led to conclude that these different appearances depended upon the presence of different substances in different parts, he tried such bodies as gum, sugar, albumen, starch, resin, &c. Separately none of these bodies gave the purple colour to the acid.

Trying them two and two together, so soon as sugar and albumen of egg were put to sulphuric acid, the purple colour was produced.

In an experiment with a fragment of the perisperme of a ripe grain in sulphuric acid the colour was produced; but besides, very curious and active motions were remarked, and little drops separated from the fragment, also purple, and moving about: these being suspected to be oil, a drop of olive oil was put into sulphuric acid, and the same movements were observed, but without the purple colour; the addition of a little sugar, however, immediately produced the colour. It was then supposed that albumen only produced colour with the sugar, because of the oil it contained; and some albuminous fibres, well boiled in water, to separate all the oil possible, gave little or no colour to sulphuric acid and sugar, though before they gave it abundantly.

Hence M. Raspail concludes that the acid may be used microscopically as a test for sugar, albumen, oil, and resin. Thus, placing a vegetable organ, fresh, or having been preserved in water, in a drop of strong sulphuric acid, if it be observed to give a yellow colour to the acid, but to yield no oily drops, and occasion no motion, then it consists either of resin or sugar; adding a drop of oil, or white of egg to it, if it become instantly coloured purple, the change proves that the substance was sugar. If no purple colour is thus produced, a drop of concentrated solution of sugar is added to a similar preparation of sulphuric acid and the substance experimented with, and if no colour is obtained, then the principle dissolved must be resin. If oily drops are obtained in these experiments, the sulphuric sugar will render them purple; if the acid and the substance produce no colour, and cause no motion, but a purple colour is obtained by sugar, then the principle present is considered as albumen; or if it be from the vegetable kingdom, gluten, which is considered as vegetable albumen.

Examining in this way both animal and vegetable tissues, M. Raspail states, that all the parts of the fœtus, of its enveloping membranes, and of the uterus itself, in the animal, correspond in chemical composition, *i. e.* in their action with sulphuric acid, with the similar parts of the vegetable creation, and that sugar accompanies the albumen around the new individual, or the point at which its formation commences in the one kingdom as well as the other.

These phenomena of coloration disappear so soon as the acid is diluted, either purposely, or by attracting water from the atmosphere; a point which of course requires attention in the prosecution of these and similar experiments.—*Bull. Univ.* A. x. 267.

25. *On the Fatty Matter of Wool.*—The following facts are taken from a long memoir by M. Chevreul, on the principal varieties of wool, having for object their immediate composition, for the purpose of ascertaining the influence of foreign matters which might be present in them M. Chevreul obtained 18 hundredths, at least,

of a fatty matter from Merino wool, which had previously been well washed in pure water. This matter is formed of two distinct principles, which differ from each other in their degrees of liquefaction. The one, at ordinary temperatures, is like wax; whilst the other, in similar circumstances, is like turpentine. Each is susceptible of forming an emulsion with water, and in this respect differs from stearine and oleine, and resembles the fatty matter of brain.

When heated with solution of potash, in such a manner that oleine and stearine would have been saponified, these substances exhibited no appearance of that change. They did not appear to contain any azote, and in that point differ from the fatty matter of brain.

It is singular, that woollen cloth which has been deprived of 18 hundredths of its weight of fatty matter, does not appear more apt to become dyed than it was before; though the contrary might have been supposed, from the known necessity of removing the grease from wool before dyeing it. Wool, which has lost its fatty matter, contains the sulphur which is observed in that which has not had the former principle separated; and in either state, the wool treated by alum and tartar disengages a little sulphuretted hydrogen.

It is this sulphur contained in wool which causes the production of colour, when the wool is heated in a solution of acetate of lead; or in acetate of alumina, containing a little acetate of lead; or still more strongly in protochloride of tin, &c.—*Ann. de l'Industrie*, i. 422.

26. *Specific Gravity of Solutions of Sugar.*—The specific gravity is taken at 64° F. The solutions are formed of one part of sugar in as many parts of water as are expressed in the following table:—

Water.		Water.		Water.	
10	1.036	6	1.057	2	1.143
9	1.040	5	1.068	1	1.230
8	1.045	4	1.080	0.5	1.332
7	1.051	3	1.105		

—*Brandes.*

27. *Ceric Acid.*—When wax is acted on by alkalies, a species of soap is formed. If this soap be distilled with dilute sulphuric acid, there passes over, according to M. Pfaff, a liquid which is acid, and peculiar, and which contains no sulphuric acid; it is powerful enough to decompose carbonates, and has been distinguished as ceric acid.

28. *Preparation of Alizarine, a colouring principle of Madder.*—The following processes and chemical points in the history of this substance are from a paper by M. Zenneck. To prepare alizarine, the madder in powder is to be macerated in cold water and filtered; the solid part mixed with a little yeast and water, fermented, then filtered and pressed; the solid residue is then to be digested in alcohol so long as a coloured tincture is obtained; the alcoholic liquors put together, distilled until three-fourths have passed over, and the remaining fourth precipitated by diluted sulphuric acid; the

red-brown precipitate is to be collected, dried, and afterwards sublimed.

Or ten parts of the finely powdered madder may be digested with four parts of ether, in a close flask; the clear solution distilled until the residue is thick like a syrup, and then this dried upon plates in the open air; the residue is to be powdered and sublimed.

The sublimation is to be effected in a watch glass, or a metallic capsule, covered by a very low cone, on to which the vapours are to be condensed; the layer of matter to be sublimed must be thin, and the distance through which the vapours have to pass as small as possible; a little cotton should be placed at the orifice in the summit of the condensing cone, and the heat applied be moderate, especially at the commencement.

Alizarine, well sublimed, is in very brilliant, diaphanous, yellowish red, acicular, quadrangular crystals; they are soft, flexible, heavier than water; have an acid taste, soften when heated, sublime at the temperature of boiling oil, and then occasion a peculiar aromatic odour. It is nearly insoluble in cold water; one pound of boiling water dissolves about one grain of it. At common temperature, one part dissolves in 210 of alcohol, and in 160 of ether. It combines with and dissolves in alkalies, forming violet solutions; and, in fact, has all the properties of an acid. It even passes to the positive pole of the voltaic pile; and when boiled with metallic zinc, causes its oxidation, and then unites with it; 100 parts combine with, and saturate 350 parts of oxide of lead; so that it has a greater saturating power than oxalic acid. Its composition per cent. is given as 18 carbon, 20 hydrogen, and 62 oxygen.—*Annalen der Physik*, 1828, p. 261.

29. *Sanguinari, a New Vegeto-Alkali.*—M. Dana obtains this substance by the following process from the *Sanguinaria canadensis*, L. called in America blood-root. The powdered root is to be digested in pure alcohol for some time; a little ammonia added to the tincture obtained, and the latter then poured into water; the brownish precipitate is to be collected, mixed with powdered charcoal, washed with boiling water, and thrown upon a filter. The mixture on the filter collected and acted upon by alcohol yields a solution, which, by evaporation, leaves the *Sanguinaria* in a pearly, white, solid state. This substance is acrid; it reddens tincture of litmus, and combines with acids, neutralizing them, and forming red salts: it is insoluble in water, very soluble in alcohol and ether, and, by exposure to air, acquires a yellow colour. It appears to exist in the plant combined with a peculiar acid.—*Hensman's Rep. der Chem. Phil. Mag. N. S.* v. 151.

III. Natural History.

1. *Natural Nitrogen Springs.*—Nitrogen gas issues in almost a pure state from the earth on John Bradt's farm, in Hoosick, just

within the limits of Rensselaer county, New York. It appears to issue from every part of a low hill, comprising four or five acres of ground; for wherever there is water it bubbles through it. It issues abundantly from the hill through three springs from the clear gravelly bottoms of each. Probably the gas accompanies the water from a considerable depth, for the springs are not influenced by the wet seasons. The underlying rock appears to be sparry limestone, but the detritus here is very deep.—*Silliman's Journal*, xv. 234.

2. *Native Sulphuric Acid.*—Professor Eaton has described the natural occurrence of sulphuric acid in large quantities, both in a diluted and a concentrated state, in the town of Byron, Genessee county, ten miles south of the Erie canal. The place has been known in the vicinity, for seventeen years, by the name of the *sour springs.* The place consists of a hillock 230 feet long and 100 broad, elevated about 5 feet above the surrounding plain; its greatest extent is north and south; it consists of an ash-coloured alluvion, containing an immense quantity of exceedingly minute grains of iron pyrites: it is mostly covered with a coat of charred vegetable matter four or five inches thick, and black as charcoal; the same kind of matter extends on all sides, from the base of the hillock over the plain. Its charred state is caused wholly by the sulphuric acid. Several holes have been dug in the hill, which now contain turbid dilute sulphuric acid; also the depressions on the meadow ground around it. The strength of the acid increases in a time of drought: when Professor Eaton examined it much rain had recently fallen, and the acid was very dilute in most places, but it was strong in some, and appeared to be quite concentrated and nearly dry in the charred vegetable coat. In this state it was diffused through the whole piece of ground, which presented the charred appearance to the depth of twelve or fifteen inches, and in some places three or four feet. But it was every where the strongest at surface.

In wet spring seasons it appeared that adders-tongue, and some other plants, flowered on this hillock sooner than on the adjoining grounds; but as soon as the spring rains began to decline, then the vegetables withered away, and appeared as if scorched.

About two miles east of this place is another sulphuric acid spring, still more remarkable in one respect. The quantity of water from the spring is in sufficient quantity to turn a light grist-mill, and yet there is so much sulphuric acid present in it, that the stream will constantly redden violets, and its water coagulate milk. Several other sour springs were mentioned as existing in the neighbourhood.

It is supposed that the sulphuric acid is produced in some way by the decomposition of the pyrites in the soil.—*Silliman's Journal*, xv. 239.

3. *Native Sulphuric Acid.*—The formation of sulphuric acid from sulphuretted hydrogen gas has been observed by M. Egidi, in a natural

grotto, in the department of Aquasanta. A powerful disengagement of sulphuretted hydrogen takes place there; the gas, in contact with the atmosphere is gradually decomposed, producing water and sulphur; sulphites are soon found, and then sulphates, and finally, sulphuric acid, which, as it passes over the walls, gradually dissolves the earth and oxides which it meets with.—*Gior. di Fisica,* 1827, p. 484.

4. *Large Rock Crystals.*—Gigantic crystals of smoky quartz are found in the vicinity of Paradise river, a few miles from Bridgetown, in Nova Scotia. They occur in the masses of granite, or imbedded in the alluvion which forms the banks of the Anapolis river. One was found weighing 120 pounds; another weighs 90 pounds, and presents gradations of tint from light topaz, through clove brown, into an opaque smoky colour, almost black. Long, slender, prismatic crystals of black schorl traverse its surface, and penetrate three or four inches deep into its solid substance. The extreme length of the crystal is 19 inches; its diameter, at the base, 12 inches; its six lateral planes are 9 inches in length to the acuminating planes, one of which, being unduly extended, is 12 inches in length, and nearly replaces the two adjoining ones.—*Silliman's Journal,* xv. 209.

5. *Extraneous matters in Quartz Crystals.*—Professor Eaton says, that the students of Rensselaer school have, during the summer of last year, found numerous quartz crystals in the calciferous sandrock, one mile north-east from the school, which were terminated by six-sided pyramids at each end, and containing anthracite coal. They found two specimens which contained a liquid, and one which had a piece of coal floating in the liquid.—*Silliman's Jour.* xv. 362.

6. *Large Crystal of Beryl.*—A crystal of beryl is mentioned in Silliman's Journal of extraordinary size. It came from Ackworth, N. H. It had no perfect termination, was nine inches in its transverse diameter, and weighed forty-seven pounds.—xv. p. 358.

7. *Nature of Vegetable Microscopical Spicula.*—Microscopical investigations have enabled M. Raspail to observe certain spiculæ in *Pandanus*, the *Orchis*, *Scilla*, &c., and to determine their nature: those above, and all those which are about $\frac{1}{10}$ of a millemeter (.004 of an inch) in length, and $\frac{1}{300}$ (.00013 of an inch) in diameter, are hexaedral crystals of phosphate of lime: the crystals of the tubercles of the iris, which are about $\frac{1}{3}$ of a millemeter (.013 of an inch) in length, and $\frac{1}{50}$ (.0008 of an inch) in width, are rectangular crystals of oxalate of lime. The magnifying powers by which these results were obtained was from 1000 to 2000 diameters. These crystals have usually been mistaken for microscopical hairs.—*Bull. Univ.* B. xv. 369.

8. *Meteoric Iron in Provence.*—M. Hericart de Thury read a

letter to the Academy of Sciences from M. Brard, in which the latter states, that there has existed a mass of iron in the commune of Lacaille, in Provence, for a long time, which there is every reason to believe is of meteoric origin.—*Oct. 13th.*

9. *Fulgorites in Westphalia.*—The very curious vitrified tubes which have been remarked in sandy soils, and attributed to the action of lightning, have been seen formed, and forming, at Senna, in Westphalia. The surface is a turfy soil, but beneath that occur various sands, which have been traced to the depth of 90 feet whilst digging for a well. Where the superficial vegetable earth has been removed the wind evinces great power over the sand, carrying immense quantities to some distance. Here and there are deep bogs; and it appears that the sand-tubes, or fulgorites, are most abundant upon the edges of these bogs, and where the sand is uncovered. These tubes, or conduits, sometimes pass downwards vertically, sometimes horizontally, but most usually in oblique directions, dividing in their course into many branches.

In proof of the opinion that these tubes are formed by lightning, MM. Brandes and Echterling, to whom the above observations are due, state, that a violent storm happened on the 29th of April, 1825; in this situation they went with other persons to the place where the storm had been active, and found the ground furrowed horizontally and vertically in such a way, that the sand at the edges of the furrows was vitrified, and formed tubes, or natural conduits, exactly like those found in other parts of this country.—*Bull. Univ.* A. x. 350.

10. *Earthquakes in Holland.*—Earthquakes have been very rare in Holland, only ten were on record previous to last year; but during 1827 as many as three occurred; one on the 23rd of February, the second on the 21st of March, and the third on the 3rd of December.

11. *Parhelia, &c.*—The following account of appearances in the atmosphere is given by a Moscow Journal:—On the 4th of February a phenomenon equally extraordinary and magnificent was observed at Kiaihta, in Siberia. At sunrise, the cold being extremely severe, luminous rays, known in Siberia by the name of the sun's ears, were perceived on both sides of that planet. At ten o'clock in the morning the rays changed into parhelions. An immense whitish column, similar to a comet's tail, issued from the sun, which had already risen to a great elevation, and took the direction of the west; there was formed throughout the whole extent of the heavens a regular circle, at the circumference of which were seen seven images of the sun, pale and without rays, situated at nearly an equal distance from the sun and each other. The sun, moreover, reflected on the atmosphere four large white circles disposed so as to form a pyramid, and of which two were circumscribed

in the above-mentioned circle, whilst the other two were in the part of the horizon opposite to the sun. It was remarkable that there should have been four circles in the large one ; but one of them was effaced by the brightness of the sun, and of another only half could be seen resplendent with the colours of the rainbow. The pheno menon continued until nearly noon.

12. *Vegetation in Air at different Pressures.*—M. Dobereiner took two equal glass vessels of 320 cubic inches capacity each ; in these were put portions of the same earth in which two portions of barley had been sown, and moistened to the same degree. The air was now exhausted from one vessel until the pressure equalled 14 inches of mercury, and in the other it was condensed until the pressure equalled 56 inches. Germination took place in equal times, and the leaflets were equally green ; but at the end of 15 days the shoots in the rarefied air were only 6 inches long, but in the condensed air from 9 to 10 inches. The former were expanded and soft, the latter rolled round the stem and solid: the former were wet on their surface, and especially at the extremities; the latter nearly dry. "I am disposed," says M. Dobereiner, "to believe that the diminution in the size of plants, as they rise on mountains into higher regions, depends more on the diminution of pressure than of heat." The phenomenon of drops of water on the leaves in rarefied air calls to mind the relation of a young Englishman, who, whilst passing through Spanish America as a prisoner, remarked, that "on the highest mountains of the country the trees continually transpired a quantity of water, even in the driest weather, the water falling sometimes like rain."—*Bib. Universelle.*

13. *On the influence of different organic and inorganic substances upon Plants, by* M. E. A. Zeller.—The experiments that had been made upon this subject have been repeated by M. Zeller, and many others added to them. He found that it was not poisons only which exerted an injurious action upon plants, but also very many other substances, such as gentian, volatile oils, valerian, camphor, rhubarb, ipecacuanha, emetic tartar, &c. Plants yielding poisons or etherial oils, faded and dried when they were made to absorb the substances produced by themselves. Narcotic substances, the bitter and volatile oils, and alcohol, exercise their influence upon the principal vessels of the plants, and then gradually extend it to the circumference of the leaves: the nitrate of baryta, emetic tartar, and many other salts, on the contrary, first act at the circumference of the leaves, then on their central parts, on the leaf stalk, and the stem. Sensitive plants lose the power of contracting their leaves by the action of laurel water, opium, and nux vomica. Camphor makes them contract, and then they no longer dilate again. Poisons do not act upon monocotyledonous and dicotyledonous plants in the same manner: many amongst the latter suffer severely. Coniferous plants

are but little sensible, and the same is the case with fatty plants, and those with hard leaves. Plants which have suffered the action of poisons without perishing, lose their leaves, and of the branches which have suffered, most of them shoot forth with new vigour. The influence of rain and dew seems advantageous to them. All salts are injurious when used in large quantity, but, when used in small quantity, they importantly assist the powers of vegetation.—*Bull. Univ.* B. xv. 369.

14. *Poisoning of Plants by their own juices.*—In consequence of the experiments made by MM. Marcet and Macaire Prinsep, on the effects of poisons on plants, the latter was induced to examine what would take place if a plant producing poison was acted upon by its own product. The experiments were made with *Datura Stramonium*, *Hyosciamus Niger*, and *Momordica Elaterium* i. Recent branches of these plants, placed in solutions of from one to five grains of their respective extracts in one ounce of water, soon faded, and died in two hours; whilst similar branches, in water containing as much gum, underwent no change. ii. Branches plunged into water mixed with the recent juice of the plants suffered in the same way. iii. Plants of the above-named kind growing in pots, when watered with solutions of the extract taken from their own branches, soon faded and died.

Hence it appears that, as poisonous animals may be destroyed by their own poison, so is the case with vegetables. M. Macaire Prinsep thinks that either the extract becomes poisonous by changes effected upon it by the air, or else that different vessels in the plant contain different products. As to the action of air, he says, that the recently expressed juice of all these plants, when put into confined portions of air, absorbed oxygen, and gave out an equal volume of carbonic acid; but, then, the juices of other plants do the same.—*Mém. de Genève.*

15. *Method of obtaining Roses of all kinds twice in the year.*—The following directions, by M. Douette Richardot, are to enable the amateur to gather as fine roses in September as he did in the preceding June. i. Immediately after the first flowering, the shrub is to be deprived of every leaf, and those branches which have borne roses cut so that only two or three buds shall remain. The cutting of the weaker branches may be in a less degree. If the weather be dry when the leaves are removed, it will be necessary to thoroughly water the stem, for several days, with the rose of the watering-pot: in this way, the sap will not be arrested. ii. Then the brush is to be used, and the rose tree well cleansed by it, so that all mouldiness shall disappear: this operation is very easy after an abundant rain. iii. The earth about the rose tree is to be disturbed, and then twenty-four sockets of calves' feet are to be placed in the earth round the stem, and about four inches distant from it. The hoofs of young

calves are the best, and give a vivid colour and agreeable perfume to the roses. These are to be placed with the points downwards, so that the cups shall be nearly level with the surface of the earth, and the plant well surrounded. This operation is to be repeated in the November following. These hoofs, dissolved by the rain or the waterings, form an excellent manure, which hastens the vegetation, and determines the reproduction of flowers. iv. Two waterings per week will suffice in ordinary weather, and they should be made with the rose of the watering-pot, so that the hoofs may be filled; but, if the atmosphere is dry, it will be necessary to water the plants every evening; and in the latter case it will be necessary, from time to time, to direct the stream of water on to the head of the tree.—*Bull. Univ.* D. x. 342.

16. *On the Plantation of Potatoe Buds.*—M. Grebel, a clergyman of Ringleben near Erfurt, made some experiments on this subject in 1826. Notwithstanding the great dryness of the weather, the produce much surpassed his expectation, since each sprout yielded from three to four pounds of potatoes, and some of the potatoes weighed nearly a pound each. He remarked that, according to his experience, the early potatoe called Hernkartoffel, by Putzsche, is the most advantageous for this method of propagation: a single germ gave sometimes from six to seven pounds of potatoes.—*Allgem. Anzeiger, Bull. Univ.* D. x. 314.

17. *Subterraneous Growth of Potatoes.*—A mixture of two parts Danube sand, and one part common earth, was laid in a layer an inch thick, in one corner of my cellar; and, in April, thirty-two yellow potatoes with their skins placed upon its surface. They threw out stalks on all sides; and, at the end of the following November, more than a quarter of a bushel of the best potatoes were gathered, about a tenth part of which were about the size of apples—the rest as large as nuts. The skin was very thin; the pulp farinaceous, white, and of a good taste. No attention was given to the potatoes during the time they remained on the sand, and they grew without the influence of the sun or light. This trial may be advantageously applied in fortified places, hospitals, houses of correction, and, in general, in all places where cellars or subterraneous places occur, being neither too cold nor too moist, and where it is important to procure a cheap, but abundant nourishment for many individuals.—*Mercure de la Souabe—Recueil Industriel,* viii. 149.

18. *On the Gossamer Spider, by* Mr. Bowman.—Several of these little insects were arrested in their flight, and placed upon the brass gnomon of a sun-dial: in a short time they prepared for their aërial voyage. Having crawled about to reconnoitre, they at last turned their abdomens from the current of air, and elevated them

almost perpendicularly, supporting themselves solely on the claws of their fore legs; at the same instant shooting out four or five, often six or eight, extremely fine webs, *several yards long*, which waved in the breeze, diverging from each other like a pencil of rays, and strongly reflecting the sunbeams. After the insects had remained stationary in this apparently unnatural position for about half a minute, they sprung off from the stage with considerable agility, and launched themselves into the air. In a few seconds after, they were seen sailing majestically along, without any apparent effort; their legs contracted together, and lying perfectly quiet on their backs, suspended from their silken parachutes, and presenting to the lover of nature a far more interesting spectacle than the balloon of the philosopher. "One of these natural aëronauts I followed," says Mr. Bowman, "which, sailing in the sunbeams, had two distinct and widely diverging fasciculi of webs; and their position in the air was such, that a line uniting them would have been at right angles with the direction of the breeze.—*Magazine of Natural History.*

It would be very interesting to know, whether the gossamer threads thrown out by these insects are in an excited state of electricity: their divergent state would seem to imply they were; for there seems to be no other natural cause which could prevent them from coming together, especially before the insect had left its resting-place. If electric, then neighbouring bodies, as the hand or branches of a tree, or a stick, &c., would attract them; but care would be required in making the experiment, from the readiness with which these threads would move upon disturbance of the air. If electric, then it would be important to know whether they were positive or negative; which their attraction, or repulsion, by a stick of sealing-wax, rubbed on the sleeve of a coat, would at once determine. It is well known that these threads are almost perfect insulators of electricity, and would retain a charged state for a long time in a dry sunny atmosphere.—*Ed.*

19. *Instinct of Spiders.*—A small spider (*Epeira Diadema*, Latreill) had spread its net between two neighbouring trees, at the height of about nine feet. The three principal points to which the supporting threads were attached, formed here, as they usually do, an equilateral triangle. One thread was attached above to each of the trees, and the web hung from the middle of it. To procure a third point of attachment, the spider had suspended a small stone to one end of a thread; and the stone, being heavier than the spider itself, served in place of the lower fixed point, and held the web extended. The little pebble was five feet from the earth. The whole was observed, and is described, by Professor Weber, of Leipsig.—*Archive für Anatomie*, 1827.

20. ***On the Sexual Instinct of Insects, by*** **J. H. Davies.**—It has

been asserted, that the circuitous flight of the butterfly tribe arises from one sex pursuing through the air the track of the other; and that, if an unimpregnated female of the *Phalæna quetcus* (egger moth) be carried in a gauze cage into the haunts of that species, numbers of the males will be attracted, so as to be easily captured. I have never had an opportunity of verifying this fact; but, from a circumstance which occurred to me during the past year, I have no doubt of its correctness.

I was engaged in rearing lepidopterous insects from the larvæ, and had a great variety of the pupæ of different species. One evening, I found a female *Sphinx ocellata* just emerged, which, in lifting from the floor, ran up my arm and round the collar of my coat: two hours after, on returning to my study from shutting some glass frames in the garden, a very fine male, of the same species, was fluttering on my shoulder, where the female had previously crawled. But a still more curious fact, which must appear almost incredible, remains to be stated. Two females of the sphinx populi were evolved. The next day I found three males in the room. As no one had entered it in the interval, nor was there apparently any mode by which they could gain access, I was somewhat puzzled to account for their appearance. The same evening, however, the mode of entrance was made apparent, by two more males, of the same species, coming down the chimney; one of which fell into a vase standing on the fire-place, where I captured it before it could extricate itself. Afterwards, upon occasion of the evolution from the pupa state of females of the *Phalæna bucephala* and *Phalæna salicis*, the windows of my study were completely besieged by males of the same species, which, upon throwing open the windows, eagerly rushed in.

21. *American Sea-Serpent.*—Mr. Samuel Mitchell has, in his "Summary of the Progress of Natural Science for the last few Years," given an amusing account of the progress of sea-serpentism. It was read before the New York Lyceum, and is inserted in the American Journal of Science, xv., p. 351, although not thought conclusive by its learned editor, Dr. Silliman. The first sea-serpent was a steam-boat, which, being established at Boston to coast along the shore, and from its powers and capabilities competent to injure the business of small boats, was described as a sea-serpent that had been seen off Nahant and Gloucester, and had probably come there to consume all the small fish in the place. This was received by many as a serious account, and believed accordingly.

Another sea-serpent history arose from the circumstance, that a small sloop, called the Sea-serpent, having been passed by another vessel, the captain of the latter, when asked, upon his arrival at home, for news, said he had seen a sea-serpent, and then described its bunches on the back, the action of its tail, and other parts; all of which being understood literally, actually appeared in print, as evidence for the existence of the animal.

Then a piece of the skin of the bony scaled pike was taken for part of a sea-serpent's hide. A speckled mother duck, with a numerous brood of young ones swimming after her in a line on Lake Ontario, was described as the serpent itself. And from such occurrences as these, perhaps, mingled with careless observation of the motions and appearances of porpuses, basking sharks, and balænopterous whales, appears to have originated every thing that has been said about American sea-serpents.

22. *Living Molluscous Animals in Diluvial Deposits.*—The following statement is by Professor Eaton, of New York:—" The diluvial deposits in the great diluvial trough, through which the Erie canal is made, contain ridges of hard, compact gravel. A remarkable ridge of this kind lies in an oblique direction across the canal, running south-westerly from the village of Rome sixteen miles west of Utica. While cutting through this ridge, the workmen found several hundred of live molluscous animals. They were chiefly of the *Mya cariosa* and *Mya purpurea* (Uni of Bruguieres and Barnes). I have before me several of the shells from which the workmen *took* the animals, *fried* and *ate* them. Mr. Brainard, who now resides at the place, and keeps a public-house on the banks of the canal, was present at the time, and will give the curious a *satisfactory* account of it. I have received several of the shells, with satisfactory assurances that the animals were taken alive from the depth of forty-two feet in the same deposit, and near the same place.

" My object is to present a case where the deposit is decidedly diluvial; consequently these animals must have lain from the time of the deluge: for the earth in which they are found is too compact for them to be produced by a succession of generations. Therefore the lives of these animals are greatly prolonged by exclusion from air and light, or their natural age is more than 3000 years.

" These fresh-water clans of 3000 years old *precisely resemble* the same species which now inhabit the fresh waters of that district." —*Silliman's Journal*, xv. 249.

23. *Polar Red Snow.*—According to the chemical examination of the red snow brought from the north by Captain Franklin, MM. Macaire Prinsep and Marcet are inclined to think it may be of animal production and not vegetable, *i. e.* to consist of animals or animalcules. The analogy of this substance to a red matter, taken from the lake of Morat, was one amongst other reasons for this opinion, and also the circumstance that gelatine (containing azote) has not as yet been found in the vegetable creation, whereas it is in this red snow.—*Bib. Universelle*, xxxix. 290.

24. *Comparison of Respiration and Combustion.*—It has usually been supposed that a comparatively small proportion of carbonic acid gas will render an atmosphere quite unfit to support respiration, and sometimes 6 per cent. has been said to be the limit of respirability. This, however, has been long known to be exaggerated; and lately M. Ferigo has had an opportunity of observing, that air charged with nearly 17 per cent. could be breathed for some hours consecutively. A well was in progress in the commune of Triuggio, in the Milanais, at the bottom of which candles would not burn: it was found, nevertheless, that men could work there; and in fact a workman continued to labour for three hours at a time, being then relieved by another workman, who had to remain for the same period, and so on. The air in this well being collected and analysed, extinguished inflamed bodies when plunged into it, and by analysis was found to contain one-sixth of its volume of carbonic acid gas.—*Giornale de Fisica*, 1827, 433.

25. *Spontaneous Human Combustions.*—That cases happen in which the human being, even when alive, undergoes a sudden destruction, as if by a consuming process, cannot be doubted; and these are now so numerous as to have induced M. Julia de Fontenelle to read a paper on the subject to the Academy of Sciences at Paris. Fifteen instances are particularly described by him, from the details of which the following general results are obtained:—

i. Generally those who have died by spontaneous combustion have indulged in excess of alcoholic liquors. ii. The combustion is almost always general, but in some cases may be partial. iii. It is rare amongst men; the women have, in almost every case, been aged. iv. The body and the viscera have always been burnt, whilst the feet, hands, and top of the head, have almost always been preserved. v. Although it is known by experience that a very large quantity of wood is required to burn a corpse, this particular kind of incineration occurs without inflaming the most combustible substances of an ordinary kind near it. vi. It has not been shewn in any case that the presence of an inflamed body is necessary to commence this kind of combustion. vii. Water, instead of extinguishing the flame, appears to give it more activity; and when the flame has disappeared, the combustion proceeds within. viii. They occur more frequently in winter than in summer. ix. The cure of general combustions has never been effected, only of partial ones. x. Those to whom it has happened have experienced a sensation of strong internal heat. xi. It is suddenly developed, and consumes the body in a few hours. xii. Those parts which are not reached by the fire are affected by sphacele. xiii. A putrid degeneration ensues which causes gangrene. xiv. The residue of this combustion is composed of greasy cinders and an unctuous fatty matter, both having a fetid odour, which is perceived at a great distance.

These results shew the inapplicability of the explanations which have sometimes been given of this effect. Considering how much fuel is required to consume a corpse, it is impossible to suppose that, in these cases, the combustion is occasioned by the inflammable nature of alcohol, or of hydrogen, which may be supposed to be evolved; and again, the ordinary products of the combustion of animal matter is a very spongy, black, shining charcoal, fetid, and incinerating only at a very high temperature, whilst in spontaneous human combustion the temperature produced is so low as not to inflame neighbouring bodies.

M. Julia de Fontenelle, therefore, considers this case of combustion to depend upon a very advanced and putrid degeneration of the system, which suddenly produces very combustible substances at the expense of the muscular fibre, &c. and these inflaming, spontaneously, (by means of the opposite electricities, &c.) cause the resulting effect. This degeneration is considered as presenting a perfect analogy with vegetable putrid fermentation and putrefaction. The putrefaction of vegetables is known to occasion the developement of so much heat as sometimes to cause their inflammation.

These human combustions do not depend upon the combination of the oxygen of the atmosphere, because, 1st., there is not sufficient heat evolved; 2nd., there is not the production of a charcoal requiring a high heat for its incineration; 3rd., there are no ammoniacal products formed. The effects appear to depend altogether upon a new arrangement of the elements previously existing in the human body.—*Bull. Univ.* C. xv. 245.

26. *Method of stopping Hæmorrhage from Leech-bites.*—Occasionally leech-bites, left to themselves, bleed excessively, even so much as to endanger life; and cases have occurred where all the usual means of stopping the issue of blood, when tried, have failed. The Marquis Ridolfi then says, that the application of the cupping-glass to the point from which the blood escapes is always successful. He uses a very small glass when the hæmorrhage proceeds from a single bite, and a larger one when it issues from several near to each other. Almost immediately on the application of the glass the blood forms a clot over the bite, which suspends the hæmorrhage. The formation of this clot takes place with great facility, even in subjects in whom the blood is very thin and aqueous. The glass is allowed to remain on for a few minutes, until the integuments become tumefied. Care must be taken in removing it not to disturb the coagulum, as only the fluid part of the blood should be emptied, and the glass is to be applied again and again, until the hæmorrhage has completely ceased.—*Med. Rep.* ii. 75.

27. *Arabian Mode of curing Fractured Limbs.*—The Orientals will never consent to have a limb cut off. Their practice is to lay the limb upon an oiled mat, after reducing the bones, and the

inclose it in a case of gypsum, or plaster of Paris, an operation which they perform much in the same way as is practised by statuaries to take the cast of a limb. They first pour the plaster of Paris under the limb, until it rises to such a height as to touch the whole lower surface, filing up all inequalities, so as to form a sort of bed, placing, at the same time, a few hollow reeds at proper distances, and in such positions as to serve to conduct away, through the plaster, any fluid that might collect in the gypsum case, from the wounds, &c. When this becomes firm, which it does in a very short time, the limb is next covered with the same plaster of Paris, so as to inclose it completely, and, on hardening, to form a light case, or plaster boot, to keep the parts in as natural a position as possible. They next make a sort of furrow, or channel, in the soft plaster, on the upper surface, to receive such vulnerary fluids, during the treatment, as they think conducive to the cure, and which filter through the gypsum, to humect the leg at pleasure. To render this upper shell more easily removed or changed during the cure if necessary to examine the state of the parts, &c., they make deep incisions into the soft plaster, both lengthwise and across, though not quite through, by means of which the upper case is removed without disarranging the limb. The firmness of the lower part, or bed, makes the removal of the whole boot practicable, should such a measure at any time be found expedient.

It will be remembered that Baron Larrey has lately practised a somewhat similar mode of treatment for fractured limbs with much success. The Baron does not make use of a case of plaster of Paris, but surrounds the limb with bandages which have been soaked in some glutinous matter, which forms nearly as firm an envelope as plaster of Paris.—*Med. Journ.* xxxii. 175.

28. *On some New Species of Fossil Hyænas, discovered in the Cavern of Lunel Viel, near Montpelier.* By MM. Jubes de Crietol and A. Bravard.—Among the fossil animals, the hyæna is one of those which has been most frequently met with in the superior strata of the tertiary formations. They have been found in France, England, Germany, Hungary, Italy and Switzerland, most frequently in the caverns which were their former dens. In some places the number of the individuals of this species is truly surprising; still more than one species has not hitherto been met with, and upon that M. Cuvier bestows the name of the fossil hyæna. This hyæna, the strongest and the largest which ever existed, approached, as appears from the dimensions of its skeleton and the form of its teeth, the hyæna of the Cape, the spotted hyena, which is also the largest and most fierce of the living ones; the fossil species, however, surpass it considerably. A second species of living hyena, smaller and less strong than the Cape hyæna, is the hyæna of the Levant, the striped hyena, which had not as yet been found in a fossil state. M. Cuvier insists principally upon this circumstance, when he says, "All the heads and lower jaws of hyænas from Germany, England, and

France are of one and the same species, and contain teeth altogether similar to that which, I may say, is applicable to them all.' Professor Buckland, who has had greater opportunities than any other person of examining fossil hyænas, has, likewise, stated that the striped one is never met with in a fossil state, so that it was not without astonishment he saw the one we showed him. We have, in fact, met with the striped hyæna, or at least a species very nearly allied to it, in the cavern of Lunel Viel, which it formerly inhabited, as well as the fossil hyæna, a denomination now insufficient, since there are at least two species of fossil hyænas; we say, at least two species, for it appears to us that there exists a third; but prior to examining the motives which induce this belief, we must detail the characters which enable us to recognize the striped fossil hyæna.

Although the skeleton of the Cape hyæna and that of the Levant have nearly the same specific characters, still there are sufficiently decided differences to distinguish one from the other: the head and the teeth, for example, offer sufficiently sure characters; but among the teeth, one in particular carries with it a certain decisive mark, sufficient of itself alone to distinguish the Cape from the Levant hyæna: it is the last molar of the lower jaw, called la carnassiére, and that is the very one which we found in a fossil state. The difference between these molar teeth in the two species is this: in the Cape, as in the fossil hyæna, this molar has two cutting lobes (*Fig.* 1.) and a tubercle at its posterior part; in the striped living hyæna, and in the individual of this species, which we found in a fossil state, (*Fig.* 2.) this last inferior molar has also two cutting lobes, but the tubercle behind is larger; and beside, an essential character, which is wanting in the hyæna of the Cape and in the fossil one hitherto known, is, that the posterior lobe has at the base of its interior face a prominent tubercle (*a*), which is found only in the striped living hyæna and in the analogous fossil one. Here, in our opinion, are very simple and easily distinguishable characteristics, which sufficiently prove the existence of a new fossil hyæna, which we shall denominate *the striped fossil hyæna*, or *hyæna of Montpelier*, in opposition to the received names of striped living hyæna, or hyæna of the Levant. We have sufficient grounds, as has been seen, for connecting our hyæna with the striped one, but we do not, on that account, absolutely declare it to be the same species; we only think it is to this last it ought to be inferred, rather than to any other, on account of the identical character of the teeth, which it is known are among quadrupeds what is most constant, and least subject to the variations so frequent in the colour of their skins and in their dimensions.

As for habits, it is probable, they must be the same with those of hyænas of the present day, only, on account of its greater height and strength, it may be presumed it was more formidable, and, perhaps, was not contented with devouring the putrid carcases of other animals, but even fiercely attacked them; for the modern hyæna, in spite of its voracity, is so great a coward, that, according to Dr. Sparmann, it is only in extreme cases it will contend with a common dog of the African planters. "Altogether, voracious as it is," says this traveller, "it dares not attack, openly, either oxen, cows, or horses, nor any other large animal, as long as they seem disposed to resist, and evince no sign of fear; but, on the other hand, it has the art to attack them unexpectedly, uttering at the same time a strange and horrible cry. It is well known," he adds, "that in dark nights they are met with round the butcheries of the Cape, *where they devour the bones*, the skins, and all that is thrown there, *carrying away with them what they are unable to eat*. But this very gluttony of the hyæna, and its disposition to consume all that comes in its way, is it not a striking instance of the prudent economy of nature? The flowery plains of the Cape would soon become hideous, and strewed with the carcases of the innumerable animals which pasture and die there in such rapid succession, if nature had not visibly charged the hyæna to clear the stage. I might have said the hyæna alone, for the lions and the tigers never eat the bones, and are not fond of carcases. We may add to these observations, that the hyæna, in spite of its astonishing voracity, is capable of supporting hunger for a long time; the cowardice with which it attacks living animals also announces that it is destined to hasten the end of every animal worn out by age, or sick, or lame; to consume the other remains and refuse of the animal kingdom; but that it is not designed to destroy the animals necessary for the propagation of their species." This description will show that the bones accumulated in the cavern of Lunel Viel, having the evident marks of teeth *, prove the habits of the hyæna of Montpelier to have been the same as those of the animal at the Cape of Good Hope, and that, like these last, they carried away and devoured the bones of every other sort of animal. The great quantity of their excrements in this cavern proves that they lived there, and that, as in Africa, they carried thither the carcases of elephants, rhinoceroses, wild boars, and stags, which inhabited the country round. They inhabited caves like those now existing. Buffon states that the hyæna takes up its abode in dark caverns, and in the clefts of rocks. Besides it is now the opinion of most philosophers, of Mr. Buckland and M. Cuvier in particular, that these hyænas lived in the caverns wherein they are found to be buried. Relative to the bones of caverns M. Cuvier writes, "All these bones and these teeth are accumulated on the ground, broken and gnawed, and on them are even found traces of the teeth by which they were broken. Likewise excrements, which have been recognised as perfectly similar to those of the hyæna, have been found there. The most abundant deposit of hyæna bones that has been seen is the cavern of Kirkdale, in Yorkshire, where their number is marvellous. It is found, nevertheless, that the habits of this ancient hyæna resemble those of the modern one; and Mr. Buckland has happily applied to his cavern of Kirkdale this passage of Busbequius, which seems to describe it. "Sepulchra suffodit, extrahitque cadavera, portatque ad suam speluncam, juxta quam videre est ingentem cumulum ossium humanorum veterianorum et reliquorum omne genus animalium." If the hyænas of Kirkdale have not heaped up human bones with those of the many herbivorous animals of which their cavern is full, it is because they found neither dead nor living men in their vicinity; and this fact may be considered as an additional proof that our species did not dwell with the animals, which I now bring to light." We may apply this remark to the cavern of Lunel Viel, which, visited by men well versed in anatomy, has never disclosed any human remains. But, nevertheless, as in other caverns, the animals found there resemble more nearly those now alive; they date from an epoch posterior to the age of the paleotheriums and anoplotheriums, to which there are at present none analogous, and the discovery of hyænas, similar to the modern ones, also confirm the principle laid down by M. Cuvier, that in the caverns the carnivori approach more nearly than elsewhere those now upon the earth. The causes then were, doubtless more similar to those now operating; and a single revolution separates that epoch from the one in which we live. It is in, fact, to this last revolution of the globe that the immense inundation, which partially filled the caverns and the clefts in the calcaire grossier (London clay,) and oolite formation, which are found in almost every part of Europe, with mud and gravel, must be attributed. These extensive currents have left in the environs of Montpelier very considerable deposits which may be easily recognised; a loose bed, a diluvial stratum which has not been covered by any other; a calcareous earth, argillaceous, of a light red colour, which is found on the surface, and abounds in flints, gravel, calcareous and quartz pebbles, from a chain of mountains which border the Mediterranean. At Cette, this diluvium rests on the oolite formation, and penetrates the clefts of the dolomite, which forms the lower part of the mountain of Cette, where it is hardened by the stalactites, and forms the osseous fissures described by M. Cuvier. Around Montpelier it rests on the upper marine formation, and the upper fresh-water calcareous formation;

---

* These marks are so well preserved and so perceptible in some pieces, that it may be known by what species of tooth they were bit. Thus traces of the incisors, the false molars, and the carnassières may be recognised. The teeth, which were not worn down, have left on the bones marks which resemble the stamp of a conical sharp punch. The traces of the incisors are like grooves, or parallel furrows; they are seen only in the spongy parts of the bone. The false molars, worn and mutilated at their point, have produced prints shaped like small stops, or semicircles, which succeed each other, because these molars, by compressing the bones, made at first many consecutive round holes, which holes finished by dividing into two portions where the tooth pierced into the bone more deeply. The carnassières, the last of the molars, left marks similar to what the repeated blows with a chisel would have done, cutting in the same place. Mr. Buckland, who examined them with us, found them exactly resembling what he had seen in other caverns, and what are at present taken by living hyænas. His experiments on this subject are too well known for us to detail them here.

farther on, towards the north-east, it rests on the calcaire grossier (London clay formation), into which it enters and forms the osseous fissures which we have discovered at Vendargars, in the forest of St. Antoine, and in the quarries of St. Julien. It is most clearly seen near the town of Lunel, where is the cavern containing the bones into which this diluvium rushed, leaving around great heaps of this very red earth, gravel and pebbles, which are to be met with in great abundance, particularly on the borders of the high road from Montpelier to Nantes, at the hill of Montregret.

Nothing, therefore, in geology appears to us better established than this opinion, that the result of the last revolution of the globe was a vast inundation which covered our continents; overwhelmed the plains and small hills with mud and gravel, filling up the grottoes and crevices, carrying with it the animals it met in its progress, and precipitating them into the pits over which it rushed, and of which many had served as dens for wild beasts; that all the animals, as the elephant, the rhinoceros, the lion, which are now found only in the torrid zone, inhabited our climate; and the discovery which we have made recently at Pezuas, of the elk and the reindeer, associated with the elephant and the hippopotamus, is an additional proof in favour of M. Cuvier's opinion, that the elephants and rhinoceroses of the ancient world were inhabitants of cold countries.

Having then announced the existence of a new fossil hyæna, and offered our reflections regarding its habits, the places it inhabited, and the causes of its destruction, we must revert to our third fossil species, observing that what we have said with respect to the first applies naturally to this one, and that we do not attribute its presence in the same cavern to any other causes than we have assigned for the last, viz., that there also it had chosen its den; in fine, that the same cause of destruction overwhelmed them at the same time, and that at an epoch anterior to any recorded in history.

After having admitted two species of living hyænas, the striped and the spotted, M. Cuvier says that there exists in the Museum of Natural History at Paris, a hyæna, but of what country it is a native he is ignorant, and which he believes to form a third species, or to be a variety of the striped hyæna. Its skin is neither striped nor spotted; it is less strong than the other two species; and its teeth closely resemble those of the striped hyæna, only that the internal tubercle of the posterior lobe of the last lower molar tooth is less sharp, less prominent than in the striped hyæna. Here, then, are teeth in which the characters are perfectly similar to those in our third species of fossil hyæna in the cavern of Lunel Viel. These teeth of the lower jaw have two cutting lobes; they have a prominence in their posterior part, and a tubercle round the base of the posterior lobe, but this tubercle is very much less than in the teeth of the striped living and fossil hyæna; and an important particular which distinguishes them also from every other species is, that this tubercle is not in the same place as in the other species; it is much more behind, and joins the other prominence, from which, nevertheless, it is quite distinct. The ledge or collar of the anterior part is scarcely perceptible. I am ignorant if all this hold good with the hyæna from an unknown country of which M. Cuvier speaks, for he is silent on this point which it would be curious to verify. In *Fig.* 3, we give a representation of this tooth, presuming that all those we have mentioned still adhere to the jaw-bones, and are preceded by many others which have all the characters of the teeth of hyænas, so that it is certain they do not belong to any other carnivorous animal, and it is only to avoid a useless multiplication of figures that we have given it singly.

This would be the place to discuss many interesting questions relative to other parts of the skeleton of the fossil hyænas of the new species: it is to be presumed that objects would not be wanting; for in the great number of bones that we have seen in the public collections of Montpellier, there are heads belonging to different species; but hoping that the researches which government is causing to be made in the cavern of Lunel Viel, will enable us to examine a greater number of objects, we shall defer speaking of them till those labours are terminated.

Although we have expressed ideas apparently very different from those which M. Marcel de Serres has published in his different memoirs on caverns and fissures containing bones, our opinion does not differ in the end from his, but in a few points which are most essential in the question we have been discussing. The important point is to consider the fissures and caverns as filled up at the same time by a general cause; and on this fact we agree with him, but we differ in this: he supposing that the animals, the remains of which are found in our caverns, were all carried thither by a current alone; while we, on the contrary, suppose, that the strange collection of these animals in the same place is principally owing to successive generations of hyænas having inhabited these caverns; thinking, however, that among the bones some may have been brought by the inundation which filled up these same caverns with mud and pebbles.

Fig. 1. The last molar tooth (carnassière) of the lower jaw of the Cape Hyæna.
Fig. 2. .... hyæna of the Levant.
Fig. 3. .... of a third fossil hyæna.

Fig. 1.

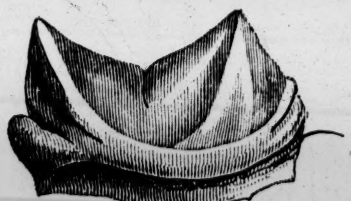


Fig. 2.

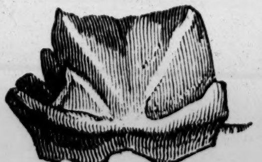
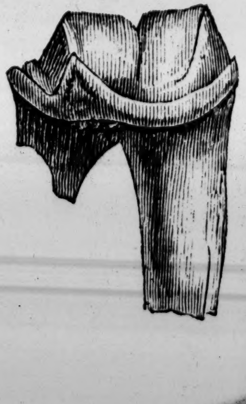


Fig. 3.



—*Mémoires de la Societé d'Histoire Naturelle.*

29. *Description of a Pastenaca (Raia Pastenaca of Humboldt), from the river Meta in South America.* By M. Roulin.

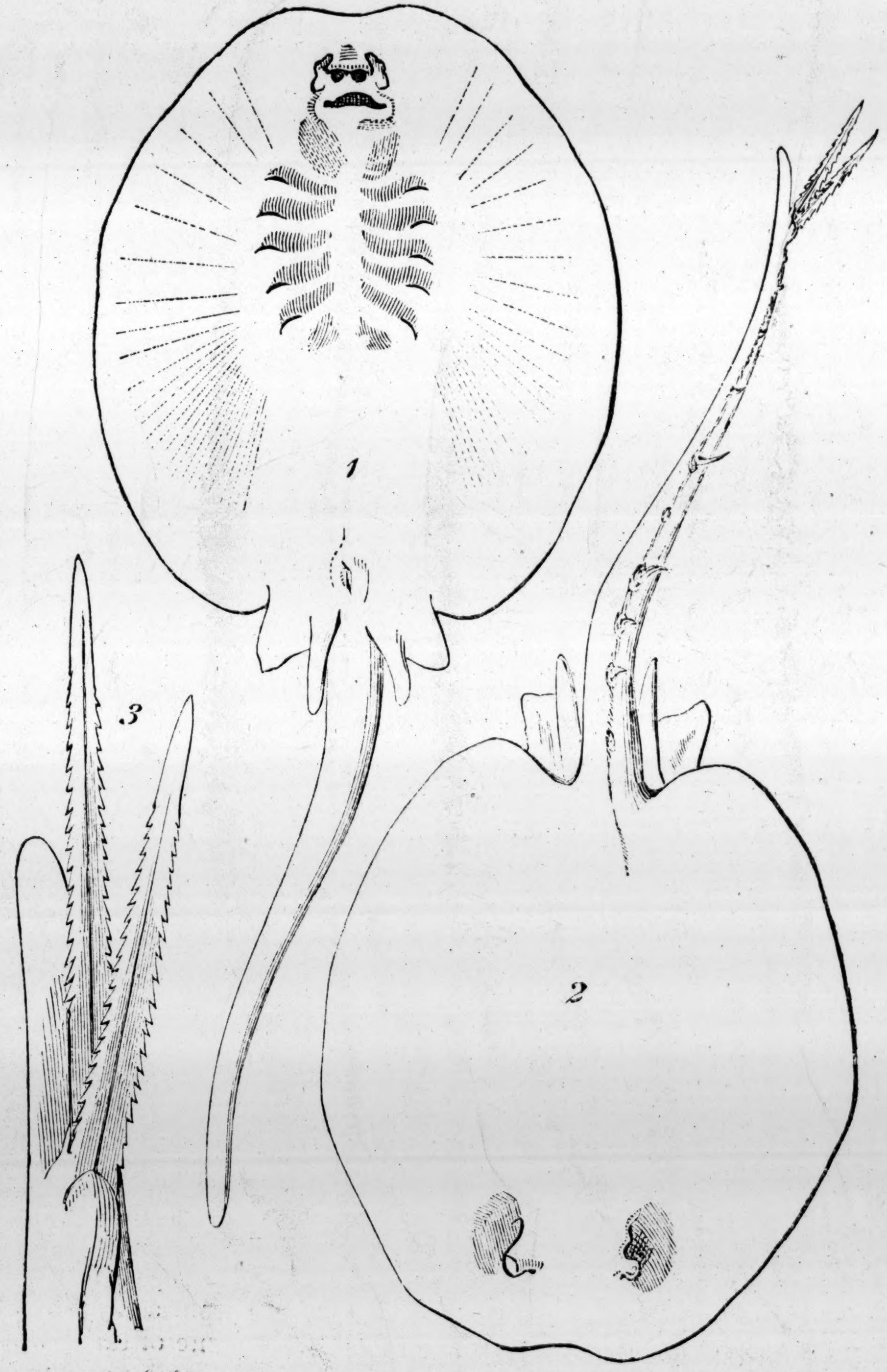


Fig. 1. The pastenaca of Humboldt, under side.
Fig. 2. Ditto ditto, upper side.
Fig. 3. Double sting of the tail of a pastenaca about one-fourth larger than that of which I have given the dimensions.

The rays, and even the cartilaginous fishes in general, with the exception of the petromyzons, the lampreys, are considered at the present day as belonging exclusively to the sea. Yet pastenacas exist in many rivers of southern America, and sometimes many different species of them are met with in the same waters. Thus, in the upper part of the Meta, in the province of Saint Martin, there is a spotted pastenaca, and also a black one, which I now make known, distinguishing it by the name of the celebrated traveller, whose labours have thrown most light upon the natural history of this country. The two pastenacas which served me for this description, were harpooned by a fisherman of the village of Giramena; in size they were exactly similar; when presented to me, they were mutilated, the tail being cut off above the sting, for fear that any one who handled them imprudently should be hurt, but the stings themselves had been preserved, they being used as points to the long arrows, which are poisoned with the Woorara. I procured one of these stings, which I have since deposited in the collection of the Museum of Natural History of Paris; the end of the tail upon which it was supported, had been stripped of its skin, so that I could not personally ascertain if this tail wanted, as they assured me it did, a fin at its extremity. The shape of the body of this pastenaca is elliptical, the back of a dark olive brown colour, marked with small black converging lines, which together form a curve, terminating in several sloping faces; the eyes are small, prominent, placed on a sort of peduncle, directed upwards, a little outwards and forwards: immediately behind, and a little outwards, there exists on each side a rather large opening, which dips under the peduncle of the eyes. The belly is very white in the middle; the mouth, curved, is furnished with blunt teeth, arranged symmetrically; in front is the opening of the nostrils, separated by a septum, covered in part by a loose flap, the wings of which, on each side, are entirely detached. Behind the mouth is the branchial apparatus, of an oval form, and occupying more than a third of the length of the body; it is formed of six double ranges of branchiæ, each having their orifice at the external part. The tail is round, slightly conical; at symmetrical distances, it has spines nearly like those of our thornback or skate, and towards the union of the posterior third with the anterior two-thirds, one or two long flattened stings, terminated by a very sharp point, and serrated in a reverse direction on both sides, which slant off to an edge: from this disposition, it results, that when the ray strikes with its sting, it enters with facility, but comes out with difficulty, making a lacerated and very painful wound, sometimes, it is said, followed by death. The fisherman had on his leg the scar of a wound from a ray; he had also heard it said that it occasioned death, but none of those whom he had known had felt from it such fatal effects: some persons had required a long time to be healed, and he was not certain if all had been so. The Indians of San Martin eat the black pastenaca; as to the other, they believe its flesh to be venomous; on which account perhaps it is that it is sometimes called raya cascabel, the crotalus ray; perhaps, also, it may be from the brown colour spotted with yellow of its back, which resembles the skin of a rattle-snake, which is very common in that province. The dimensions of the largest ray, in English feet and inches, are as follow:—

	ft.	*in.*
Longest diameter of the body, *i. e.* from the anterior part of the circumference to the anus . . .	1	4·7
Smaller diameter of the ellipse . . .	1	0·9
From the anterior part of the nose to the mouth .	0	2·0
From the mouth to the anterior part of the branchiæ .	0	1·8
Greater diameter of the branchial apparatus . .	0	4·6
From the posterior part of the branchial apparatus to the anus	0	7·6
Circumference of the tail at its insertion . .	0	3·9
Length of the conical appendages . . .	0	3·1
——— anal fins	0	4·0
Distance between the eyes . . .	0	2·8
Diameter of the eye	0	0·75
Greater diameter of the foramina . . .	0	.1·6
Less . . .	0	.1·0
Thickness of the body in the middle . . .	0	.3·0

All the fishermen assured me that there were much larger rays than those of which I have given the representation.—*Annales des Sci. Nat., Jan.*

Dr. Roulin having passed six years in South America, during which time he bestowed much attention on subjects of natural history, we should feel much hesitation in differing from his opinion; but as only two pastenacas, and but the sting of one, fell under his observation, we take the liberty of suspecting that he has fallen into what is by no means an unprecedented error, in attributing to this fish a double sting. In the marine species this weapon is annually cast, and as it happens frequently that the new spine has attained considerable size before the old one has been shed, the pastenaca is occasionally found with two, in which state it has been considered improperly as a distinct species.—*Note by the Editor.*

30. *Description of a New Species of Ovula from the Indian Ocean.* By M. Duclos.—Ovula Punctata. O testâ ovato-oblongâ, inflatâ, albâ, utrinque subrostratâ, striatâ, rubro-punctatâ; labro marginato; columellâ anterius concavâ.

This small and unusually elegant shell is particularly remarkable for the delicacy of its striæ and its dorsal punctuation, both arranged differently from what has hitherto been met with in the different species composing the genus ovula. Its shape is an oblong oval inflated; it is white, the back ornamented with six round reddish points placed uniformly two and two; the right lip bevelled off and finely dentated within; the left, or columellar, is very smooth and concave, in the form of a groove. This species, which is only 0.28 of an inch long, is one of the prettiest of the genus. It is found in the Isle of Bourbon.—*Mém. de la Société d'Hist. Nat.*

METEOROLOGICAL DIARY for the Months of December, 1828, January, and February, 1829, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For DECEMBER, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Monday....	1	43½	46	29.60	30.10	WbN	NE
Tuesday ...	2	24	43	30.23	30,18	EbN	SE
Wednesday.	3	30	52	29.97	29.90	SW	SW
Thursday ..	4	41	53	29.90	30.00	W	SW
Friday	5	35	50	30.00	29.90	SW	SE
Saturday...	6	34	52	29.71	29.69	SW	SW
Sunday	7	40	51	29.39	29.48	SW	S
Monday....	8	38	45	29.27	29.00	W	NE
Tuesday ..	9	31	41	29.40	29.70	W	W
Wednesday.	10	33	50	29.88	29.90	SW	SW
Thursday ..	11	38	49	29.70	29.93	SW	WbS
Friday.....	12	36	52	30.10	30.10	WbS	SW
Saturday...	13	41	54	30.10	30.16	SW	SW
Sunday	14	46	51	30.25	30.25	SE	E
Monday....	15	36	49	30.17	30.00	E	SE
Tuesday ...	16	36	50	29.91	29.81	SW	SW
Wednesday.	17	43	52	29.57	29.64	W	SW
Thursday ..	18	38	56	29.23	29.35	SW	SSW
Friday	19	38	58	29.44	29.70	SW	W
Saturday...	20	44	53½	29.80	29.80	WbS	W
Sunday	21	50	56	29.83	29.83	SW	SW
Monday ...	22	48	56	29.88	29.84	SW	WSW
Tuesday ...	23	45	52	29.80	29.63	WSW	S
Wednesday.	24	41	51	29.50	29.37	SE	SE
Thursday ..	25	36	57	29.30	29.40	W	W
Friday.....	26	28	43	29.40	29.40	ESE	SE
Saturday...	27	33	41	29.54	29.62	SE	EbS
Sunday	28	36	45	29.70	29.90	E	W
Monday ...	29	35	44	30.16	30.19	W	SSW
Tuesday ...	30	30	45	30.19	30.09	S	SSW
Wednesday.	31	39	50	29.89	29.79	SW	W

For JANUARY, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Thursday ..	1	35	47	29.70	29.70	W	W
Friday	2	33	45	29.70	29.84	NW	W
Saturday...	3	32	41	29.80	29.82	W	NW
Sunday	4	35	45	29.48	29.42	NW	NW
Monday....	5	31	38	29.62	29.80	NW	NNW
Tuesday ...	6	26	36	29.90	29.90	N	N
Wednesday.	7	30	40	29.92	29.94	NE	NE
Thursday ..	8	30	39	29.84	29.80	ENE	NNE
Friday.....	9	30½	42	29.79	29.60	NNE	NE
Saturday...	10	33	37	29.53	29.54	NNE	NE
Sunday	11	30	37	29.71	29.83	SE	E
Monday....	12	30	39	29.92	29.92	NE	NE
Tuesday ...	13	33	40	29.95	30.00	NE	NE
Wednesday.	14	27	40	30.00	29.97	NbW	M
Thursday ..	15	33	38	29.85	29.73	NE	NE
Friday.....	16	29	31	29.53	29.52	EbN	E
Saturday...	17	26	32	29.59	29.64	E	E
Sunday	18	13	31	29.84	29.84	W	WbN
Monday....	19	11	32	29.92	30.00	NW	NW
Tuesday ...	20	18½	31	29.98	29.90	WbN	NW
Wednesday.	21	23	28	29.84	29.80	NE	E
Thursday ..	22	19	30	29.67	29.60	E	E
Friday	23	19½	27	29.52	29.48	E	EbN
Saturday...	24	16	29	29.50	29.50	NW	NW
Sunday	25	12	28	29.50	29.48	SE	SW
Monday....	26	15½	40	28.92	28.92	ESE	S
Tuesday ...	27	31½	39	28.92	28.98	SSE	S
Wednesday.	28	32	40	29.20	29.37	W	WbN
Thursday ..	29	28	38	29.40	29.40	W	NE
Friday.....	30	30	39	29.37	29.53	NE	N
Saturday .	31	31½	38	29.92	30.10	N	NE

For FEBRUARY, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday	1	21	35	30.30	30.30	NE	SE
Monday....	2	16	31	30.35	30.38	SE	SW
Tuesday ...	3	13	33½	30.38	30.38	ESE	WSW
Wednesday.	4	25	41	30.28	30.17	W	W
Thursday ..	5	38	45	30.00	30.08	NW	NNW
Friday	6	34	43	30.10	30.10	SW	SW
Saturday...	7	40	43	29.30	29.45	W	NW
Sunday	8	39	44	30.17	30.15	NNE	SW
Monday ...	9	35	45	30.15	30.12	NW	NW
Tuesday ...	10	40	45	30.15	30.19	NW	W
Wednesday.	11	41	52	30.05	60.00	NW	NW
Thursday ..	12	40	46	30.00	29.95	W	NW
Friday.....	13	40	44	29.95	29.95	NW	W
Saturday...	14	40	50	29.90	20.90	NW	W
Sunday	15	39	50	29.90	29.86	SW	W
Monday .. .	16	43	49	29.80	29.65	SW	SW
Tuesday ...	17	36	44	29.62	29.62	W	NE
Wednesday.	18	33	38	29.70	29.68	E	SE
Thursday ..	19	32	48	29.52	29.43	SE	S
Friday	20	31	49	29.43	29.43	W	WbS
Saturday...	21	37	53	29.08	28.98	E	SW
Sunday	22	40	43	29.00	29.17	NNE	NE
Monday ...	23	32	39	29.31	29.38	E	EbS
Tuesday ...	24	27	39	29.33	29.34	EbS	NE
Wednesday.	25	32	41	29.72	29.82	EbN	EbN
Thursday ..	26	31	41	29.94	29.81	ESE	EbS
Friday	27	35	46	29.90	30.00	WNW	NW
Saturday...	28	27	39	30.18	30.25	ESE	SE